SCIENTIFIC

RESEARCHES,

EXPERIMENTAL AND THEORETICAL,

IN

ELECTRICITY, MAGNETISM, GALVANISM,

ELECTRO-MAGNETISM, AND ELECTRO-CHEMISTRY.

WITH COPPER-PLATES.

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PREFACE.

Although it is seldom of much consequence to the generality of readers to be informed what were the motives or circumstances that caused the appearance of any literary performance, there are certain persons who place so much importance in matters of this kind that their opinions of the character of a work are formed by these alone; and, as it is a matter of considerable consequence to many authors that the motives which stimulated them to usher their labours into public notice should neither be misrepresented nor misunderstood, they are justified in taking the best means of security against misapprehension by placing them before their readers in a clear and unequivocal form.

The circumstances which led to the appearance of the present volume are several and various. Some of them are similar to those laudable incitements that have actuated scientific inquirers, in all ages, to place before the world the results of their respective investigations collectively, and as a compact body of scientific information originating from their own developments of physical truths, together with such theoretical and practical inferences as appeared to be legitimately derivable from them. Other motives for collecting the present series of scientific labours from the oblivious pages of Journals in which many of them originally appeared, arose from a retrospection and close examination of the various subjects, both individually and collectively, to which they had been devoted; from which it appeared that, as some of the facts they had developed had already been taken advantage of and turned to account in a practical capacity, it was possible that the whole might derive an enhanced value if properly and systematically arranged according to the order of their relations to each other, and associated in one volume, so as to be consulted either separately or collectively, without the inconvenience of having to search for them amongst the several volumes in which they were necessarily insulated from each other amongst a mass of scientific matter to which they had no relation whatever.

Moreover, when thus scientifically assembled, and associated with a few other kindred facts not before published, together with occasional notes of explanation, there seemed some reason for supposing that, such a collection of Original Researches, in the most interesting branches of experimental physics now cultivated,

would meet with a friendly reception amongst all those who can appreciate labours of this description. It appeared probable, also, that as a great portion of these Researches have been devoted to the cultivation of Electro-Magnetism and Magnetic-Electricity, and their developments form a prominent feature in the present aspect of those sciences, a well arranged collection of the several facts, introduced by a faithful sketch of the history of what had previously been accomplished by other investigators in the same fields of inquiry, might be studied with advantage as a substitute for an elementary treatise, embracing the principal phenomena hitherto brought to light, especially as no such treatise has hitherto appeared in the English language; whilst, at the same time, an opportunity would be afforded of distinguishing the *extent* and *character* of these Researches from those undertaken by other individuals who have taken active parts in cultivating the same departments of physics, most of whom rank amongst the highest philosophers of the age.

Such were the circumstances that, some years ago, incited me to the preparation of the present volume; and I was further stimulated in the undertaking by the kindness and advice of several friends who favoured me with their names as subscribers should I proceed with it. Soon after the plan was arranged, however, a series of unforseen circumstances, occasioned, principally, by a severe attack of illness, which occasioned my confinement for several months, and incapacitated me for pursuing my professional avocations for a long time afterwards, completely arrested all further proceedings with the work; and although it ultimately got into the hands of an excellent printer, who has done all in his power to forward it, the impaired state of my health has retarded its progress and prevented its earlier completion

The contents of the volume are divided into six Sections: the first gives an Historical Sketch of the progress of Electro-Magnetism, till nearly the close of the year 1823, which is about the time that my own investigations were first made public through the medium of the press, so that this Section will be found a convenient introduction to the following divisions of the work, which consist exclusively of original matter.

Section II. presents an Abstract of my own discoveries, investigations, observations, &c. chronologically arranged in the order of their respective dates. This Section will be found useful to scientific readers, who are generally desirous of knowing the dates of discoveries as well as to whom they belong. References to the Scientific Journals in which the several subjects originally appeared are also given, which will relieve the reader from any unnecessary surmise respecting the correctness of the dates. The Abstract also refers to those other parts of the work in which the particulars of each subject are fully detailed. A few explanatory remarks also appear in this Section, on such circumstances as became incidentally connected with the subjects.

Section III. consists of a series of twenty-six Memoirs on the principal subjects of investigation, both experimental and theoretical; together with a description of an

original set of Electro-Magnetic apparatus, presented to the Society of Arts, in the year 1825. In order that the subjects contained in this Section may be studied to the best advantage, they are methodically and scientifically arranged, according to their relations with each other, and illustrated by a series of excellent copper-plate engravings.

Section IV. contains a series of observations on the Aurora Borealis; with such theoretical inferences as seemed derivable from the various circumstances connected with, and attendant on, several displays of that beautiful meteor.

Section V. relates a few important circumstances connected with Thunder-Storms, with observations on the employment of Electrical Kites, &c.; and

Section VI. contains a collection of Miscellaneous subjects, which could not be conveniently interwoven with the Memoirs.

With respect to the extent of real utility of this series of Researches it is not my province to offer an opinion. Like all other scientific discoveries, many of those related in this volume may possibly have to undergo a long, silent probation before they be turned to any account beyond that of expanding the regions of philosophical speculation, and affording new facilities for propagating a knowledge of the sciences with which they are respectively connected. In some few cases, however, I have probably been more than usually fortunate in developing facts that have already become of great value to society.

The electro-magnetic apparatus, represented by Plates IV. and V. very shortly after their public announcement in the *Transactions of the Society of Arts*, superseded, at the lecture table, nearly all other forms that had previously been employed. The soft iron electro-magnet also, represented in the same plates—the properties of which are clearly described in the fourth Memoir—has entered into the structure of every form of modern telegraph; and in most of them it is the force of that magnet alone that produces every movement in the telegraphic apparatus.

Without offering an opinion respecting the efficiency of the different forms of telegraph now claiming the attention of the public, I may be permitted to observe, for general information, that the deflections of magnetic needles—the only signals of some telegraphs of note—would be much more effectively accomplished by the employment of electro-magnets than by a coil of wire only. Several applications of electro-magnets, for this purpose, are represented in Plate VII., but that shown by Fig. 8 has an obvious advantage over the others. If the electro-magnets in that figure be made of soft iron wire, and not too large, they will deflect a needle at greater distances from the *same* battery than any coil-wire alone can do; and the action on the needle may be still further exalted by combining the forces of the ferruginous magnets with those of the coil-wire. By introducing the coil-wire of the bell apparatus to the circuit, the signals would be addressed both to the eye and the ear. The remarks at page 192, will show the advantage of permanent steel magnets over those of electro-magnetic origin.

The electro-magnetic *Coil Machine*, represented by Fig. 7, Plate XII, has already found more general employment amongst medical men than any other form of electrical apparatus whatever. Its external appearance has certainly undergone some change, more to enhance its elegance than its usefulness; but the principles upon which it was first constructed remain unaltered.* A variety of modes for opening and closing the battery circuit have been devised by different persons, which is one indication, at least, of the importance of the apparatus. It is not my business, however, to particularize either the inventors or their inventions, though I may be permitted to say, that the coil machines now made by Mr. Dancer, Optician, Manchester, are the most elegant that I have seen.

The *Magnetic Electrical Machine*, represented by Figs. 1 and 7, Plate XII, has some years since found its way into the specification of a patent, by Mr. Woolrich, of Birmingham, for the purpose of electro-gilding and silvering, notwithstanding my having published the same plan a long time previously, and pointed out its superiority over that in which Voltaic batteries are used. The first announcement of my apparatus was in a paper read before the Royal Society, June 16th, 1836 (see twelfth Memoir), in which it was stated that I had coated metals from metallic solutions by means of the magnetic electrical apparatus; and in a small work on *Electro-Gilding and Silvering*, published March 1st, 1842, I particularly stated the application of the same machine in the process of gilding and silvering in the following words:—"It is now more than seven years since I contrived a *Magnetic Electrical Machine*, that produced *continuous* electric currents, by means of which I coated metals with tin, copper, &c. and I have employed the same machine to great advantage in silvering, gilding, and platinizing various kinds of metal of inferior value; and I have no doubt that in this capacity the magnetic electrical machine will become generally useful. I have produced good electrotypes on a small scale by its employment." I mention these particulars to show that the original intention of placing this apparatus in the hands of the public, and alike available to all artizans wishing to employ it, can *not* be cancelled by any attempt to monopolize it by means of a patent of subsequent date.

I may also mention the immediate advantages, in the structure and operations of Voltaic batteries, that have been derived from the discovery of the superiority of *rolled* zinc over *cast* zinc, whether amalgamated or otherwise. Many new forms of Voltaic battery have appeared since the first announcement of these facts in the year 1830 (see Abstract 12, page 39, also fifth Memoir, page 146), in all of which the inventors have availed themselves of the advantages alluded to.

* Having been informed by several medical gentlemen, that, in some cases, it is desirable to transmit the pulsatory currents through those parts of the patient to which they are applied, in *one and the same* direction, the following simple modification of the apparatus will be the means of accomplishing that object. Let the coil-wire be all of one piece, and upwards of one thousand feet long, with branch wires, if thought necessary, for regulating the electric force. The discharging apparatus of course, must always be situated between the usual medical directors. By these means no current can pass through the Patient but that which occurs on opening the battery circuit.

I wish the theoretic views recorded in this volume could have been relied on with the same degree of confidence as the experimental facts, and as easily rendered useful to society as the particular discoveries and inventions above enumerated; this pleasure, however, I entertain but slender hopes of ever enjoying. Theoretical speculations, however plausible they may appear, have generally a long and tedious probation to suffer before the principles on which they are founded attain a sufficient degree of credit to become firmly established in the records of science. The most plausible theories of Electricity were broached a hundred years ago, and are still struggling in their probationary transits; and those that have been applied to terrestrial Magnetism, though commenced at a much earlier period, and modernized by recent developments of experimental data, are still lingering in uncertainty and doubt. Hence it is not to be expected that the theoretical views which I have taken respecting Electro-Magnetism and Magnetic-Electricity should be universally acknowledged, independently of severe trials and a long probation—and it is even possible that other views may be developed that would appear to throw into the shade every vestige of those which I have myself opened and dwelt upon with satisfaction. Nevertheless, as the principles upon which those theories are based are applicable to the development, and explanatory of the mode of excitement, of every known phenomenon in their respective branches of physics, the certainty, simplicity, and exactness of their predications are not likely to be superseded by others, and can never be obliterated by time.

Moreover, as these theoretical principles are not mystified by any idea of occult qualities, they are fortunately countenanced by the following excellent remarks of Sir Isaac Newton:—"To tell us that every species of things is endowed with an occult quality, by which it acts and produces manifest effects, is to tell us nothing: but to derive two or three *general principles* of motion from phenomena, and afterwards to tell us how the properties and actions of all corporeal things follow from those manifest principles, would be a very great step in philosophy, though the causes of those principles were not yet discovered."* Although the principles I have set forth may probably not apply to "all corporeal things," they apply very exactly to all those things within their own physical province.

I know of no philosopher more capable of close reasoning on electro-magnetics and magnetic-electrical physics than Professor Page, M.D., who, from a due consideration on the production of phenomena, has drawn the following inference:—"The effects produced by the breaking of a primitive elementary current are due solely to *magnetic excitation*, and have no connection with the primitive, except that of cause and effect. In fact, strictly speaking, the results thus observed are not *secondary*, but *tertiary* phenomena—the secondary production being the development or neutralization of *magnetic forces*. And, as Mr. Sturgeon has very ably set forth in his beautiful theory of *magnetic lines*, in the present state of our knowledge, it is indispensible to the

* Treatise on Optics, third edition, page 377.

explication of the reciprocal action of Magnetism and Electricity to suppose the existence of a secondary intervening medium, whether the coil conductor act with or without the co-operation of ferruginous bodies."*

The principles of Electro-Chemistry were first broached by Sir Humphry Davy; but their application to the dissolution of individual metals in acid or alkaline menstrua, have, I believe, originated in my own Researches, and seem to satisfy every particular case. The thermo-electric experiments of simple bodies, detailed in the second Memoir, are the only ones of the kind on record. They have afforded an ample manifestation of the influence of metallic crystalline structures in giving direction to electric currents when such movements are occasioned by change of temperature, and tend to prove an inequality of electric tension on the opposite surfaces of each metallic film.

The observations on the Aurora Borealis, recorded in the fourth Section, will be useful chronological data in the history of the meteor; and the theoretical views that I have taken respecting its cause, may possibly give a new impulse to observation and philosophical inquiry.

Perhaps the most valuable part of Section V. is the *Caution to Experimenters with Electric-Kites*, but the observations on the oblique discharges of lightning, both in that Section and the twenty-first Memoir, ought to claim the attention of every one engaged in the erection of lightning conductors. The marine lightning conductors described in that Memoir, appear to me to be well calculated to carry off oblique discharges of lightning without injury to the rigging; but what progress that system of conductors may make in practice would be difficult to determine. I have heard of a very large merchant-ship being furnished with a similar system of conductors, but their efficacy can only be known from circumstances that are of uncertain occurance, and cannot be predicted.

I cannot conclude this preface without acknowledging the obligations I am placed under by the great care with which the proof sheets have been examined and corrected by my excellent literary and scientific friend, John Just, Esq., of Bury. It is not to be expected, however, that the work is entirely free from errors; but I have reason to think that they will be of such a trifling nature as to produce no mistake in the description of the subjects. I have introduced no critical remarks, but such as seemed necessary for the guidance of my readers; and I hope that I have paid a due respect to the valuable labours of other scientific investigators.

WILLIAM STURGEON.

Manchester, March, 1850.

* Silliman's Journal of Science and Art, for July, 1838. Also Annals of Electricity, &c., vol. 3, page 481.

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SECTION I.

HISTORICAL SKETCH OF ELECTRO-MAGNETISM FROM ITS COMMENCEMENT UNTIL THE YEAR 1823.

The power exercised by the loadstone over pieces of iron was well known to philosophers of the remotest antiquity; but, beyond the sympathy thus evinced for iron, little or nothing was known of the general properties of the magnet: its polarity and tendency to assume certain directions, with respect to the poles of the earth, long remained concealed from observation, not being discovered till about the twelfth century. A phenomenon so remarkable and important as that of the directive tendency of the magnetic needle was well calculated to arouse the immediate attention of philosophers, and to inspire them with ardour in pursuit of the discovery of a rational explanation of its cause, which, as was reasonable to expect, was soon attempted in a variety of ways.

Some of the earliest hypotheses that were advanced may be placed amongst the wildest chimeras that could possibly suggest themselves to the mind of man: the more sober-minded philosophers, however, were enabled to trace the influencing power to the action of the earth, considered in the capacity of a grand natural loadstone, whose predominating magnetic forces reside in the polar regions; by virtue of which the obedient needle is constrained to assume certain directions, according to its residentiary locality with respect to the governing terrestrial magnetic poles, which had already been supposed to be permanently situated at some short distance from the poles of the axis of rotation.

This notable hypothesis, the production of sound analogical reasoning, by Gilbert, was founded on the miniature attractions and repulsions exhibited by magnets on each other—phenomena easily traced to, and assimilated with, the action incessantly and

universally displayed over every part of the surface of the earth, on a needle unreservedly submitted to its magnetic polar influence. The plausible and, from the then early stage of discovery, very natural conclusion thus arrived at, was for a long time universally received as the true theory of terrestrial magnetism; but the variation of the needle, at the same place of observation, was still, however, productive of impediments in the way to truth, which required fresh efforts of the mind to reconcile the theory to the phenomena: but the complexity of the problem served only to increase the ardour of emulation, and new creations of vivid genius and fertile imaginations were variously manifested in the several attempts at its solution.

Descartes, finding his vortices inapplicable to the present case, imagined that the transportation of iron from one place to another, and the growth of new iron within the earth, where there was none before, might be the cause of the needle's change of position. Kircher tried the problem by the supposition of multitudes of flexible magnetic fibres; and Du Fay by means of ferruginous capillary tubes, which he ingeniously furnished with valves, to insure one direction only for the circulation of an imaginary magnetic effluvium; whilst Canton thought of solving the problem by means of subterranean fires, although he ascribed the diurnal variation to the heat of the sun. Volcanos and other restless agents, both real and imaginary, were resorted to, and marshalled, in this clashing of opinions, to re-enforce particular hypotheses, whose authors, ambitious of fame, appeared to be determined, at all hazards, to attempt a transplacement of the terrestrial magnetic poles, by agencies which, even to themselves, must have appeared inefficient and preternatural.

The famous theory of Dr. Halley, which worked with four magnetic poles and a revolving loadstone nucleus in the earth, though probably as inaccurate as the rest, seems for a while to have had a considerable degree of ascendency; and if it did not completely paralyze its predecessors, its ingenious machinery was eminently calculated to throw into the shade the phantom forces of subterranean fires, ferreous transportations, and all the perturbating agencies of former hypotheses; which, even had they existed in reality, would have been too irregular in their actions, and too limited and local in their performances, to assume the dignified character of the prevailing universal powers which govern the uniform motions of the magnetic needle.

The magnetic effects which had been known to be produced by lightning,* and Franklin's discovery of the identity of lightning with electrical discharges artificially

* During a thunder storm at Wakefield, in July, 1731, the lightning struck the iron frames of a chamber window of a house belonging to a tradesman, in which were packed up for a foreign market a great number of knives and forks. It broke the iron frames and all the glass of the window, and afterwards struck the box in which the cutlery was packed, split it into pieces, and scattered its contents in every direction all over the room. "On gathering up these knives and forks, some of them were found to be melted, others snapped in sunder; others had their hafts burned, others not; but what was most remarkable, on laying them on the counter, where there were iron nails, rings, &c., it was observed, that when any of them were taken up, there hung a nail, or a ring, at the end of each of them: most of them were tried and found to do the same." Some of these

produced, were sufficient materials for novel ideas respecting the nature of magnetic action ; and the *Aurora Borealis*, also considered an electrical phenomenon, and known to disturb the magnetic needle, gave an auxiliary impulse to fresh inquiries, at that time directed in search of the identity of Electricity and Magnetism. Philosophers were still further encouraged in this pursuit by the striking analogies which are so remarkably characterized in the attractions and repulsions of the two powers. Similar electrized bodies display a tendency to recede from each other ; and so do those which are similarly magnetic. Dissimilarly electrized bodies will mutually approach each other; and so will those parts of bodies which are dissimilarly magnetic. Thus far the analogy would appear to be complete ; but it will be shown, in a subsequent part of this work, that these attractions and repulsions are but the preludes to the display of other phenomena, by which a discrepance in the electrical and magnetic powers is strikingly manifested ; and it must be acknowledged, however reluctantly, that, notwithstanding the most curious and interesting phenomena have been developed, and even a new science established, by prosecuting these experimental investigations, not one step more has been gained towards identifying those parental elementary forces, which, conjointly, give birth to electro-magnetic phenomena.

In the earliest inquiries in the search of Electro-Magnetism, philosophers employed every implement of investigation of which they could, at that time, avail themselves. The electrical machine and batteries of jars were the most efficient apparatus then known ; and the most formidable of them were brought into requisition, from which shocks the most violent they could produce were communicated to steel bars, of every shape and size that the experimenters could invent, to favour their views and shorten the path to discovery.

articles were magnetized to such " a degree as to take up large nails, packing needles, and other iron things of considerable weight. Needles placed on a pewter dish would follow the knife or fork, though held under the dish, and would move along as the knife or fork was moved, with several other odd appearances." (*Phil. Transactions*, 1733.)

" On January 9th, 1748-9, the new ship *Dover*, bound from New York to London, being in lat. 47 deg. 30 min. north, and lon. 22 deg. 15 min. west from London, met with a very hard storm of wind, attended with thunder and lightning, as usual, most part of the evening, and sundry large *comazants*, as they are called, overhead ; some of which settled on the spindles at the top-masts heads, which burnt like very large torches ; and at nine in the night a single loud clap of thunder, with lightning, struck the ship in a violent manner, which disabled Captain Waddell, and great part of the ship's company, in the eyes and limbs. It struck the main-mast about two-thirds up, almost half through, and stove the upper deck, one carling, and quick-work—part of which lightning got in between decks, started off the bulk head, drove down all the cabins on one side of the steerage, stove the lower deck and one of the lower deck main lodging-knees. Another part of it went through the starboard side, without any hurt to the ceiling or inside plank, and started off from the timbers four outside planks, being the wale upwards—one of which planks, being the second from the wale, was broken quite asunder and let in : in about ten or fifteen minutes time there were nine feet of water in the ship.

" It also took the virtue of the loadstone from all the compasses, being four in number, all in good order before—one in a brass box, and the other three in wooden boxes. The hanging compass in the cabin was not quite so much disabled as the rest : they were at first very nearly reversed, the north to the south ; and, after a little while, rambled about so as to be of no service. The storm lasted five days : they lost the main-mast and mizen-mast, and almost all the sails ; and arrived at Cowes, Isle of Wight, the 21st of January, in a very shattered condition." (*Phil. Transactions*, 1749.)

Besides these two remarkable instances of the magnetic effects of lightning, there are several others, of a like nature, on record.

The discovery of the identity of lightning with Electricity* called forth the whole energies of the electrical world, which, as if by the power of magic, were immediately directed to the particular branch of study occasioned by this important event; and experiments multiplied in abundance, and from every quarter, to ratify the discovery of Franklin. Experimenters vied with each other in imitating the splendid lightning of the heavens, by increasing the powers of their machines and augmenting the size and number of their jars. At length Cuthbertson brought forward his majestic double plate machine, the energies of which, combined with the immense Teylerian battery of jars,† produced such tremendous electrical explosions as could hardly be called an imitation, but lightning and thunder in reality.

Amidst this grand display of electrical refulgence, philosophers were not inattentive to the subject of Electro-Magnetism; and the great facilities that were afforded in their inquiries, by the important improvements in the electrical apparatus, and the immense quantity of electrical force which could at any time be commanded, inspired hopes bordering on the certainty of success. The subject was therefore pursued with an ardour proportionate to the interest it presented by almost every Electrician of the day.

Success for a while seemed to attend the labours of those arduous experimenters who were engaged in the pursuit, by the production of magnetic polarity in bars of steel submitted to electrical discharges. The electric fluid, in these experiments, was transmitted through the bars in the direction of their length; but the magnetic poles thus produced were soon found to have no reference whatever to the direction of the electrical discharge: a north pole would sometimes appear at that extremity of the ferruginous bar which was directed towards the positive side of the jar, and sometimes at that placed towards the negative side; thus placing the mind of the experimenter under the most unenviable impressions—on the one hand, *hope* lingering in the distance, and, on the other hand, trembling on the brink of despair. Ultimately, however, it was discovered that the results of all these laborious investigations had no superiority over those usually produced by the blow of a hammer, or other mechanical action, which could agitate, to a sufficient degree, the particles of the steel bars—during the trembling motions of which the polarization is facilitated by an enhanced susceptibility of arrangement of the residentiary magnetic matter natural to the metal, whilst under the influence of terrestrial magnetic forces.

When a soft iron bar is placed in the position in which the dipping-needle seeks to repose, its magnetic matter becomes arranged by the influence of terrestrial magnetic action; and its polarization is similar to that of the needle, with respect to their

* Franklin formally announced his opinion of the identity of lightning and electricity, in the year 1749; and the first successful experiments in verification of the justness of the hypothesis were made in France, by M. d'Alibard, on the 10th of May, 1752.

† This renowned battery was made by Mr. Cuthbertson, in the year 1786: it consisted of 100 glass jars, each of which was covered with five square feet of coating on each side; so that the discharge was made from 500 square feet of coated surface.

positions. This magnetic condition, however, is as transient as the position of the iron, and fluctuates in energy and polar locality with every movement of the bar. With hardened steel the polarization is not so easily accomplished: the magnetic energies of the earth are too feeble to vanquish the obstinate retention of the quiescent metal in which the magnetic matter is naturally imprisoned. Tremulous agitation, however, from whatever cause it may proceed, slackens the retentive powers of the steel, and liberates the obsequious element to the influence of terrestrial magnetic force: the vibrating bar thus becomes a magnet, the retention of whose polarity is progressively sealed by the gradual decline of its trembling motions.

Such were the general results of a long and arduous experimental inquiry, by the employment of the common electrical apparatus: the polarity produced in the steel by the electrical shocks being the proper effects due to terrestrial magnetic action—for that end of the bar which pointed towards the north during the electrical discharge invariably became endued with the same kind of polarity as that displayed by the north end of the needle, and the south end of the bar polarity of the opposite kind. Indeed, in some of the experiments of Father Beccaria, a famous Italian philosopher, in which the electrical discharges were transmitted through the axis of the bars, whilst placed in an east and west position, the whole of the north side of each bar exhibited the one kind of polarity, and the whole of the south side the other kind—still conformable to the laws which governed the results obtained from the other experiments.

Another experiment of Father Beccaria, the result of which appears to have been truly electro-magnetic, ought ever to be recorded in connection with the history of the science; and it is sincerely to be lamented that its ingenious author, who, in other instances, manifested such a fund of electrical knowledge and keen penetration as scarcely to be excelled by any of his contemporaries, had not on this occasion taken notice of the singularity of the phenomenon which he had developed, and persevered in the path of inquiry which now seemed to open before him.

In the experiment in question, the electrical discharge was transmitted through a few inches of watch spring, in a *transverse* direction, and the Magnetism thus produced was much more powerful, and the polarity at the extremities of the steel more decidedly arranged, than when the electrical discharge was transmitted lengthwise, or from one end to the other. Such would be the result upon the true principles of Electro-Magnetism, provided the electric current proceeded across the steel on one side of it only; or even if the current were unequally divided on the two surfaces, as was probably the case in this particular experiment. But as the inquiry was no further prosecuted at the time, the germ of Electro-Magnetism, which was thus gradually unfolding by the experiments of the celebrated Italian Electrician, was permitted to collapse, and to remain in concealment, amongst the mysterious arcana of nature, during more than another half century of years.

Father Giambatista Beccaria was one of the ablest Electricians of his day, and took an exceedingly active part in this particular inquiry; and, although he did not take advantage of the peculiarity of the facts which his experiments developed, and which led him to the very brink of Electro-Magnetism, his conclusions are very remarkable. "Are not these peculiar effects of the electric fire, with respect to Magnetism," said he, "so may proofs which corroborate my former conjectures, that the peculiar magnetic force observed in *loadstone* is to be attributed either to atmospherical or subterranean strokes of lightning; and that the *universal systematical* properties of magnetic bodies are produced by an universal systematic circulation of the electrical elements?" This is precisely the principle which, in fifty years afterwards, became the basis of M. Ampere's hypothesis of all magnetic action.

The discovery of Galvanism, about the year 1793, commenced the most splendid era in the history of Electricity; and the invention of the Voltaic battery* furnished philosophers with a new implement of electrical research, and from sources, till then, entirely unknown. The energies of this, the most formidable of electrical apparatus, which for a while were solely directed to the stimulation of languid animal functions and arresting the progress of disease, were discovered, by Messrs. Nicholson and Carlisle, to accomplish chemical decomposition in a very remarkable manner; and, in the hands of a Davy, the Voltaic battery was destined to develope those mysterious forces of nature which, from the beginning of time, had enchained the atoms of matter in chemical combination—to introduce novel modes of investigation, to establish a new epoch, and adorn the most brilliant era in the history of chemical philosophy.

Notwithstanding this unparalleled career in chemical advancement, the pursuit of Electro-Magnetic phenomena, which had become languid from the almost univeral failure of preceding inquiries, acquired a new impulse from the advancing importance of Voltaic Electricity, which rekindled the ardour of philosophers, and gave a fresh glow on the prospects of discovery. Thus reanimated, experimenters again entered the field of research, and applied the powers of the Voltaic battery in every possible way that their inventive imaginations could suggest. But, notwithstanding the most profound philosophical talent and experimental dexterity that the world could produce were exercised in these investigations, the results of this new mode of inquiry appeared, for a long time, even less favourable to the discovery of the object in view than those previously obtained by the employment of the electrical machine.

The idea of the identity of Electricity and Magnetism, so long entertained, being still kept in view, the experimental investigations with the Voltaic battery were similar to those which had proved fruitless with the other class of electrical apparatus. Electrical currents were directed through bars of steel from one end to the other,

* The Voltaic Pile, which was the original form of this apparatus, became known in England in the year 1800, by a letter from Professor Volta himself, to Sir Joseph Banks, President of the Royal Society.

which produced a diffused kind of magnetic action; but no definite polarity was obtained, nor any law developed, that could enable the operator to predict with certainty in which extremity of the bar there would be a north or a south pole.

It appears, however, that other metals than those of a ferruginous character were sometimes operated on by the Voltaic battery, with a view to discover Electro-Magnetism. M. Ritter,* a Bavarian philosopher, stated that he had succeeded in magnetizing pieces of gold by Voltaic Electricity, which retained their polarity for some considerable time. The pieces of gold particularly mentioned are a louis d'or and a gold needle: the latter is said to have obeyed the terrestrial magnetic influence for several months, displaying a *directive* quality, similar to that of the compass needle. M. Ritter likewise asserted that "a needle, composed of silver and zinc, arranged itself in the magnetic meridian, and was slightly attracted and repelled by the poles of a magnet; and that a metallic wire, after being exposed in the Voltaic circuit, took the direction of *north-west* and *south-east*, or nearly at right angles to the magnetic meridian"—a position which, as is now well known, would be assumed by an unconstrained wire carrying an electric current.

M. Ritter, who thus pursued his inquiries to within the borders of Electro-Magnetism, had long been assiduously engaged in these investigations; and as early as May, 1805, he communicated to the Royal Academy of Sciences of Munich, the results of a series of experiments which had been directed to this particular object. The following are the conclusions arrived at by this ingenious philosopher:—†

"1. Every magnet is equivalent to a pair of heterogenous metals united together: its different poles represent, as it were, different metals.

"2. Like them, it gives electricity: that is to say, one of the two poles the *positive* electricity, and the other the *negative*.

"3. By following the same process, a certain number of magnets, as well as a certain number of pairs of metals, afforded electricity; and in this manner the electricities afforded by the poles of different magnets have been successfully indicated by the electrometer.

"4. By means of these electricities, one of these batteries of magnets, accordingly as it is more or less strong, produces upon dead and living bodies all the phenomena which are produced by the pile of Volta of the common kind, and of the same force.

"5. The experiments which prove this, show that, in magnetized iron, the south pole gives *positive* electricity, and the north pole *negative* electricity; but that, on the contrary, in magnetized steel, the north pole affords the positive electricity, and the south pole the negative.

"6. The same inverse disposition is also observed with regard to the polar oxidability of the magnetized body in which this change is produced by magnetism. In

* Annales de Chimie, tome 64, p. 80.

† Taken from Professor Millin's Magasin Encyclopedique.

magnetized iron the south pole is most oxidable, and the north pole the least; whereas in magnetized steel the north pole is most oxidable, and that of the south least.

"7. By considering the earth as an immense magnet, these results might serve to explain various phenomena of nature, such as physical difference between the two hemispheres, the *Aurora Borealis* and the *Aurora Australis.* In fact, after what has been just stated, the earth, considered as a magnet, may be taken as an equivalent to an immense pile of Volta, of which the poles are on one side sufficiently closed by the waters of the ocean. And the action of this pile must produce, and has produced, the greatest chemical changes in the materials of the earth—changes which must have differed according to the poles, and of which pile the poles at the other extremity have always such an abundance of Electricity as to cause its splendour to appear in radiations in the vast spaces of the heavens."

Such were the views of this ingenious philosopher in an early period of Voltaic Electricity; and, although from their apparent extravagant character they have not commanded much attention, they certainly approach very closely to the hypothesis of terrestrial magnetism, now very generally adopted; and his subsequent experiments, already noticed, are the first on record in which *purely* electro-magnetic effects were produced by the Voltaic battery—although they by no means claim for their author the honour of discovering the laws of Electro-Magnetism.

In the year 1807, soon after Ritter's experiments became known, John Christian Œrsted, professor of Natural Philosophy and Secretary to the Royal Society of Copenhagen, published a work, in which appeared some hints corresponding pretty closely with the views entertained by the Bavarian philosopher. M. Œrsted's hypothesis supposes that the characteristic difference in the electric and magnetic phenomena arises from the different degrees of tension of the primary element producing them—the highest degree of tension being essential for the display of purely electric phenomena: a lower degree gives birth to the phenomena of Galvanism, and the lowest tension of all becomes productive of the magnetic class of phenomena.

Such was the state of the progress in these interesting investigations in the year 1807, and in this condition it was destined to slumber till the close of 1819, when the same indefatigable Danish philosopher most triumphantly succeeded in accomplishing the discovery of Electro-Magnetism. This important event, the result of the most diligent scientific inquiry, opened a new field of research, the fertility of which was soon manifested by the great variety of interesting facts that were speedily brought to light. Honourable emulation amongst experimental philosophers was never more nobly manifested, nor more successful in its career than on this eventful occasion. Novel facts, from various quarters, succeeded each other with a degree of rapidity unparalleled in the history of science; and philosophy became speedily enriched and expanded by the establishment of a new department of experimental physics.

The discovery of Electro-Magnetism was made known to the scientific world through the medium of a printed Latin essay, of which the following is a translation, as it appeared in Thompson's *Annals of Philosophy, &c.* for October, 1820:—

"The first experiments respecting the subject which I mean at present to explain, were made by me last winter, while lecturing on Electricity, Galvanism, and Magnetism, in the University. It seemed demonstrated by these experiments, that the magnetic needle was moved from its position by the Galvanic apparatus, but that the Galvanic circle must be complete, and not open—which last method was tried in vain, some years ago, by very celebrated philosophers. But as these experiments were made with a feeble apparatus, and were not, therefore, sufficiently conclusive, considering the importance of the subject, I associated myself with my friend Esmarck, to repeat and extend them, by means of a very powerful Galvanic battery, provided by us in common. M. Wleugel, a Knight of the Order of Danneborg, and at the head of the pilots, was present at and assisted in the experiments. There were present, likewise, M. Hauch, a man very well skilled in the natural sciences; M. Reinhardt, Professor of Natural History; M. Jacobsen, Professor of Medicine; and that very skillful chemist, M. Zeise, Doctor of Philosophy. I had often made experiments by myself, but every fact which I had observed was repeated in presence of these gentlemen.

"The Galvanic apparatus which we employed consists of twenty copper troughs, the length and height of which is 12 inches, but the breadth scarcely exceeds $2\frac{1}{2}$ inches. Every trough is supplied with two plates of copper, so bent that they will carry a copper rod, which supports the zinc plate in the water of the next trough. The water of the troughs contained 1-60 of its weight of sulphuric acid, and an equal quantity of nitric acid. The portion of each zinc plate sunk in the water is a square whose side is about 10 inches in length. A smaller apparatus will answer, provided it be strong enough to heat a metallic wire red hot.

"The opposite ends of the Galvanic battery were joined by a metallic wire, which, for shortness sake, we shall call the *uniting conductor*, or the *uniting wire.* To the effect which takes place in this conductor, and in the surrounding space, we shall give the name of the *conflict of Electricity.*

"Let the straight part of this wire be placed horizontally above the magnetic needle, properly suspended, and parallel to it. If necessary, the uniting wire is bent so as to assume a proper position for the experiment. Things being in this state, the needle will be moved, and the end of it next the negative side of the battery will go westward.

"If the distance of the uniting wire does not exceed three-quarters of an inch from the needle, the declination of the needle makes an angle of about 45 deg. If the distance be increased, the angle diminishes proportionately. The declination likewise varies with the power of the battery.

"The uniting wire may change its place, either towards the east or west, provided it continue parallel to the needle, without any other change of effect than in respect to its quantity. Hence the effect cannot be ascribed to attraction; for the same pole of the magnetic needle which approaches the uniting wire, whilst placed on its east side, ought to recede from it when on its west side, if these declinations depended on attractions and repulsions. The uniting conductor may consist of several wires, or metallic ribbons, connected together. The nature of the metal does not alter the effect, but merely the quantity. Wires of platinum, gold, silver, brass, iron, ribbons of lead, of tin, and a mass of mercury, were employed with equal success. The conductor does not lose its effect, though interrupted by water, unless the interruption amounts to several inches in length.

"The effect of the uniting wire passes to the needle through glass, metals, wood, water, resin, stoneware, stones; for it is not taken away by interposing plates of glass, metal, or wood. Even glass, metal, and wood, interposed at once, do not destroy, and indeed scarcely diminish the effect. The disk of the electrophorus, plates of porphyry, a stoneware vessel, even filled with water, were interposed with the same result. We found the effects unchanged when the needle was included in a brass box filled with water. It is needless to observe that the transmission of effects through all these matters has never before been observed in Electricity and Galvanism. The effects, therefore, which take place in the conflict of Electricity are very different from the effects of either of the Electricities.

"If the uniting wire be placed in a horizontal plane under the magnetic needle, all the effects are the same as when it is above the needle, only they are in the opposite direction; for the pole of the magnetic needle next the negative end of the battery declines to the east.

"That these facts may the more easily be retained, we may use this formula,—the pole *above* which the *negative* Electricity enters is turned to the *west; under* which, to the *east.*

"If the uniting wire is so turned in a horizontal plane as to form a gradually increasing angle with the magnetic meridian, the declination of the needle *increases*, if the motion of the wire is towards the place of the disturbed needle; but it diminishes if the wire moves further from that place.

"When the uniting wire is situated in the same horizontal plane in which the needle moves by means of the counterpoise, and parallel to it, no declination is produced either to the east or the west; but an *inclination* takes place, so that the pole, next which the negative Electricity enters the wire, is *depressed* when the wire is situated on the *west* side, and *elevated* when from the *east* side.

"If the uniting wire be placed perpendicularly to the plane of the magnetic meridian, whether above or below it, the needle remains at rest, unless it be very near

the pole; in that case the pole is *elevated* when the entrance is from the *west* side of the wire, and *depressed* when from the *east* side.

"When the uniting wire is placed perpendicularly opposite to the pole of the magnetic needle, and the upper extremity of the wire receives the negative Electricity, the pole is moved towards the east; but when the wire is opposite to a point between the pole and the middle of the needle, the pole is moved towards the west. When the upper end of the wire receives positive Electricity, the phenomena are reversed.

"If the uniting wire is bent, so as to form two legs parallel to each other, it repels or attracts the magnetic poles according to the different conditions of the case. Suppose the wire placed opposite either pole of the needle, so that the plane of the parallel legs is perpendicular to the magnetic meridian, let the eastern leg be united with the negative end, the western leg with the positive end of the battery: in that case the nearest pole will be repelled either to the east or west, according to the position of the plane of the legs. The eastmost leg being united with the positive, and the westmost with the negative side of the battery, the nearest pole will be attracted. When the plane of the legs is placed perpendicular to the place between the pole and the middle of the needle, the same effects recur, but reversed.

"A brass needle, suspended like a magnetic needle, is not moved by the effects of the uniting wire. Likewise needles of glass and of gum-lac remain unacted on.

"We may now make a few observations towards explaining these phenomena.

"The electric conflict acts only on the magnetic particles of matter. All non-magnetic bodies appear penetrable by the electric conflict; while magnetic bodies, or rather their magnetic particles, resist the passage of this conflict. Hence they can be moved by the impetus of the contending powers.

"It is sufficiently evident from the preceding facts, that the electric conflict is not confined to the conductor, but dispersed pretty widely in the circumjacent space.

"From the preceding facts, we may likewise collect that this conflict performs circles; for without this condition it seems impossible that one part of the uniting wire, when placed below the magnetic pole, should drive it towards the east, and when placed above it towards the west; for it is the nature of a circle that the motions in the opposite parts should have an opposite direction. Besides, a motion in circles, joined with a progressive motion, according to the length of the conductor, ought to form a conchoidal or spiral line; but this, unless I am mistaken, constitutes nothing to explain the phenomena hitherto observed.

"All the effects on the north pole above mentioned are easily understood, by supposing that negative Electricity moves in a spiral line, bent towards the right, and propels the north pole, but does not act on the south pole. The effects on the south pole are explained in a similar manner, if we ascribe to positive Electricity a contrary motion and power of acting on the south pole, but not on the north. The agreement

of this law with nature will be better seen by a repetition of the experiments than by a long explanation. The mode of judging of the experiments will be much facilitated if the course of the Electricities in the uniting wire be pointed out by marks or figures.

"I shall add to the above, that I have demonstrated, in a book published five years ago, that heat and light consist of a conflict of the Electricities. From the observations now stated, we may conclude that a circular motion likewise occurs in these effects. This, I think, will contribute very much to illustrate the phenomenon to which the appellation of polarization of light has been given.

"JOHN CHRISTIAN ŒRSTED.

"*Copenhagen*, *July* 21*st*, 1820."

M. Ampere, a celebrated French philosopher, was amongst the first who entered this new field of research, and he very shortly announced some capital discoveries in Electro-Magnetism. From his reasonings on the experiments of Œrsted, he was led to conclude that electric currents ought to exhibit some action on each other, which, on trial, he soon found to be the fact. In a memoir which, on the 25th of September, 1820, was communicated to the Royal Academy of Science, Ampere showed the conclusions at which he had then arrived, which were as follow:—

"1. Two electric currents attract one another when they move parallel and in the same direction, and they repel one another when they move parallel and in opposite directions.

"2. It follows, therefore, that when the metallic wires, through which the currents are transmitted, can only turn in parallel planes, each of the two currents tends to bring the other into a situation where it may be parallel to it, and in the same direction.

"3. These attractions and repulsions are absolutely different to the attractions and repulsions of common Electricity.

"4. All the phenomena discovered by M. Œrsted, and which I analyzed and reduced to two general facts, in my first memoir, are embraced by the law of two electric currents; admitting that a magnet is only an assemblage of electric currents, produced by the mutual action of the particles of steel, analagous to that of the elements of a Voltaic pile, and which move in planes perpendicular to the line which joins the two poles of the magnet.

"5. When the magnet is in the situation which it tends to take by the action of the terrestrial magnet, these currents have a direction opposite to that of the apparent motion of the sun; and hence, when we place a magnet in a contrary position, so that the poles which point to the poles of the earth are of the same name, the currents will be found in the direction of the apparent motion of the sun.

"6. This law embraces the phenomena of the ordinary action of magnets.

" 7. It embraces also the phenomena of terrestrial Magnetism, by supposing electric currents in planes perpendicular to the direction of the dipping needle, and which move from east to west.

" 8. There is no difference between the poles of a magnet, than that one of them is found to the right and the other to the left of the electric currents, which gives to the steel the magnetic property."

With these electrical habiliments, Ampere has given the last fashion in which the theory of terrestrial Magnetism has appeared; which, by the advantage of novel materials unknown to Beccaria, is an ingenious renovation of the views taken by the illustrious Italian philosopher about fifty years before. But, notwithstanding the discoveries of Ritter, who entertained similar views, and the adscititious elements introduced by Ampere, it is a remarkable fact that the theory of the French philosopher, at the time it was framed, had no known advantages over its Italian predecessor. The electrical currents in steel magnets, even now, are not known to have an existence, and no source of action had then been discovered to which the supposed electrical currents, essential to the earth's display of Magnetism, could be traced.

The circumstance which has given the most plausible appearance to Ampere's Electro-Magnetic theory of the earth, is the subsequent discovery of Thermo-Electricity, by Dr. Seebeck, of Berlin, by which it is shown that electric currents can be produced in bodies by an unequable temperature of their different parts. This simple process of exciting electric currents, when transferred to the vast apparatus of nature, opens to view the most magnificent theory of terrestrial Magnetism that the mind can possibly conceive. The sun would thus become the exciting agent, whose uniform tide of heat, sweeping the tropical zone, would be productive of an immense westerly circumflowing electrical flood, and thus convert the terrestrial ball into a grand thermo-electric magnet.

At the same meeting of the French Royal Academy of Science, to which Ampere's communication was made, M. Arago also brought forward some novel and interesting facts. This eminent philosopher had discovered that the connecting wire of a Voltaic battery would forcibly attract, and even lift up, iron filings; and that, by plunging it into a heap of filings, they would cling round it on every side, with their longest dimensions directly across the wire, forming a cylinder of magnetic particles of iron. This interesting fact gave rise to the idea of communicating permanent magnetic polarity to steel sewing needles, which was also accomplished by M. Arago. For that purpose the needles were introduced to the axis of a hollow spiral of copper wire, as represented by Fig. 2, Plate A, the two ends of which, c and z, were in connection with the poles of the battery; and the electric current, thus made to pass several times round the enclosed needles, excited in them magnetic polarity. M. Arago also produced similar effects by transmitting electrical discharges, through the spiral wire, from Leyden jars.

About the same time that these discoveries were made in France, Sir Humphry Davy was engaged in similar investigations in London. This excellent philosopher discovered that a current of Electricity would magnetize small sewing needles, whether that current was produced by the Voltaic battery or by the discharge of the Leyden jar. He also discovered that a wire, carrying an electric current between the poles of a Voltaic battery, would attract iron filings; and observed their transverse arrangement on every side of the wire, forming cylindric masses in precisely the same manner as in the experiments of M. Arago. Having command of extensive Voltaic batteries, our English philosopher employed them in a variety of ways in these interesting investigations. In one of his earliest experiments, one hundred pairs of four-inch plates were employed; and, on bringing the connecting wire near to some iron filings strewed on paper, they were immediately attracted by the wire, and adhered to it in considerable quantities, forming a mass round it ten or twelve times the thickness of the wire: on breaking the communication, they instantly fell off, proving that the magnetic effect depended entirely on the passage of the Electricity through the wire.

Sir Humphry says, "I ascertained, by several experiments, that the effect was proportional to the quantity of Electricity passing through a given space, without any relation to the metal transmitting it: thus, the finer the wires the stronger the Magnetism. A zinc plate, of a foot long and six inches wide, arranged with a copper-plate on each side, was connected by a very fine wire of platinum, according to Wollaston's method; and the plates were plunged an inch deep in diluted nitric acid. The wire did not sensibly attract fine steel filings. When they were plunged two inches, the effect was sensible; and it increased with the quantity of immersion. Two arrangements of this kind acted more powerful than one; but when the two were combined, so as to make the zinc and copper plates but parts of one combination, the effect was very much greater. This was shown still more distinctly in the following experiment. Sixty zinc plates, with double copper plates, were arranged in alternate order, and the quantity of iron filings which a wire of a determinate thickness took up observed: the wire remaining the same, they were arranged so as to make a series of thirty. The magnetic effect appeared more than twice as great: that is, the wire raised more than double the quantity of filings."

The Magnetism produced by Voltaic Electricity appeared to Sir Humphry Davy exactly in the same ratio as the heat (the wire transmitting it remaining the same); and, however great the heat of the wire, its magnetic powers were not impaired. This was distinctly shown in transmitting the Electricity of twelve batteries of ten plates each of zinc, with double copper arranged as three, through fine platinum wire, which, when so intensely ignited as to be near the point of fusion, exhibited the strongest magnetic effects, and attracted large quantities of iron filings, and even small steel needles, from a considerable distance.

The acuteness and sagacity displayed by Sir H. Davy, during his brilliant career of discovery in the path of Electro-Chemistry, were no less manifested in his Electro-Magnetic investigations. From inferences derived from his own experiments alone, and without any knowledge of the pursuits of the French philosophers, Sir Humphry was led into the same paths of research, and to the development of the same facts as those that had distinguished the investigations of MM. Arago and Ampere. The magnetic character of the connecting wire, which our philosopher had ascertained, led him to suppose that if several parallel wires were to connect the poles of a battery at the same time, they would all become magnetic alike, though in a less degree than in a single wire; and this was found to be the case. "When four wires of fine platinum were made to complete a powerful Voltaic circuit, each wire exhibited its Magnetism in the same manner, and steel filings on the sides of the wires opposite attracted each other.

"As the filings on the opposite sides of the wire attracted each other in consequence of their being in opposite magnetic states, it was evident," to the sagacious mind of Davy, "that if the similar sides could be brought in contact, steel filings upon them would repel each other. This was very easily tried with two Voltaic batteries, arranged parallel to each other, so that the positive end of the one was opposite the negative end of the other: steel filings upon two wires of platina, joining the extremities, strongly repelled each other. When the batteries were arranged in the *same* order, *i. e.* positive opposite to positive, they attracted each other; and wires of platinum (without filings) and fine steel wire (still more strongly) exhibited similar phenomena of attraction and repulsion, under the same circumstances."

It will be observed by these extracts, that the attractions and repulsions of electric currents, discovered by Davy, develop the same law as that shown by the experiments of Ampere. In consequence of being enabled to produce magnetic polarity in steel needles, Sir H. Davy proposed a plan for magnetizing steel bars, whether straight or of the horse-shoe form, by lightning: the bars were to be attached, crosswise, to lightning rods, being the best position for being polarized by the electric discharges passing through the rod. In some of his experiments* by the Leyden battery, Sir Humphry placed several sewing needles on the edge of a circular card, making a polygon, through the centre of which, and at right angles to the plane of the card, the conductor passed. When the discharge was made through this conductor, every needle forming the sides of the polygon became magnetic; each displaying a north and a south pole, the north pole of one needle being in connection with the south pole of the next, at every angle—the polar arrangement in the whole series being the same,

* This series of experiments of Sir H. Davy were announced in a letter to W. H. Wollaston, M.D., P.R.S., dated Lower Grosvenor-street, Nov. 12th, 1820; but most of them were made in the preceding October.—See transactions of the Royal Society for 1821.

and in strict accordance with the laws of deflection discovered by Œrsted. Davy varied his experiments in divers ways, but the principal results were those hitherto described.

Besides the facts already ascribed to Ampere, that philosopher showed that when the connecting wire of a Voltaic battery was formed into an open ring, or a rectangle, and so situated as to have freedom of motion, the plane of the ring would place itself at right angles to the magnetic meridian; and, by another ingenious arrangement of the ring, or rectangle, he found that its plane had a tendency to a position at right angles to the direction in which the dipping needle seeks to repose. These capital facts, in which electric currents manifested a decided tendency to place themselves at right angles to the earth's magnetic axis, were viewed with great interest by the author of their development, because of their appearing highly favourable to his hypothesis of the Electro-Magnetic character of the earth.

The ingenious apparatus of M. Ampere, for showing the action which two electric currents exercise on each other, is represented by Fig. 9, Plate A. Upon two pillars of brass, *op oq*, which rise vertically from a baseboard, hangs a moveable rectangular wire F D C E, furnished with pivots at E and F, which rest in shallow cavities on the tops of the pillars, which are bent inwards at *p* and *q*, for the purpose of giving freedom of motion to the suspended rectangle. The ends of the rectangular wire are fixed to the cross piece of light wood, E F, which is in the axis of motion, and is furnished with a vertical wire and a sliding ball, H, as a counterpoise to the pendulous rectangle. Another wire, N N, is fixed in two short brass pillars, in a position parallel to the lower part, C D, of the rectangle, and in the same horizontal plane. By this arrangement, one electric current can be transmitted through the wire, N N, and another through the rectangle, F D C E: when these currents flow through the parallel parts, D C and N N, of their respective circuits, and in the *same* direction, as indicated by the arrows, an attraction takes place, and the moveable wire, D C, is drawn towards the fixed wire, N N; but when the currents run through those parts of the wires in *opposite* directions, the part C D is repelled from N N.

Fig. 8, Plate A, is a representation of the apparatus employed by Ampere, for showing the *inclination*, or the position, with respect to the magnetic dip, which electric currents assume, when under the influence of terrestrial Magnetism. When the plane of the moveable rectangular wire, A B C D E F, is adjusted at right angles to the direction of the compass needle, and an electric current, from a Voltaic battery, is transmitted through it in the following manner, it is immediately put into motion, and, after a few vibrations, settles at right angles to the line of dip. The exterior ends of the wires of the battery are placed in the cups S and Z, which are partly filled with mercury. If the positive end of the battery be connected with S, the current will flow from the mercury in that cup, by means of the short bent wire, to the brass pillar,

o q; thence to the horizontal pivot at *q*, and along the moveable wire in the direction A B C D E F G; from G it will pass through the pivot on the top of the pillar, P, and from that pillar, through the other bent wire, to the mercury in the cup, Z, which is connected with the negative end of the battery. The lozenge-shaped piece, *l l*, is of light wood, and serves to keep the rectangular wire in its proper form.

Fig. 3, Plate A, represents a peculiar shaped spiral conducting wire, employed by Ampere, for showing its magnetic properties when conducting an electric current. The wire is wound on two pieces of straw, or two pieces of quill, having its extremities pointed, and terminating in two cups, containing mercury; and suspended by the upper one at Z, in the same manner, as shown in Fig. 4, which is another form of spiral sewed to a circular card, and the subsequent invention of Professor Moll, of Utrecht. When the battery connections are completed at C and Z, the current flows through the long spiral, Fig. 3, which becomes magnetic-polar at its extremities, *n* and *s*, similar to that of a compass needle: so that the axis, *n s*, places itself in the magnetic meridian. Both these spirals display active magnetic powers when approached by a steel magnet. When exposed to the Magnetism of the earth, the plane of the flat spiral, Fig. 4, assumes a position at right angles to the magnetic meridian.

In Thompson's *Annals of Philosophy*, for November, 1820, Professor Œrsted announces a second series of investigations, in which he had discovered some novel and interesting facts. He states, that "the magnetic effects do not seem to depend upon the intensity of the Electricity, but solely on its quantity. The discharge of a strong electric battery, transmitted through a metallic wire, produces no alteration in the position of the magnetic needle. A series of interrupted electric sparks acts upon the needle by the ordinary electric attractions and repulsions, but, as far as can be perceived, the sparks produced no electro-magnetic effect. A Galvanic pile, composed of 100 discs, of two inches square each metal, and of paper moistened with salt water, to serve as a fluid conductor, is likewise destitute of sensible effects on the needle. On the other hand, we obtain the effect by a single Galvanic arc of zinc and copper, having for a conductor a liquid possessed of great conducting power: for example, of one part sulphuric acid, as much of nitric acid, and sixty parts of water; we may even double the quantity of water without much diminishing the effect. If the surface of the two metals be small, the effect is likewise small; but it augments in proportion as we augment the surfaces. A plate of zinc, of six inches square, plunged into a vessel of copper containing the above described liquid, produces a considerable effect; but an arrangement of this kind, in which the zinc plate is 100 inches square, acts upon the needle with such force that the effect is very sensible at the distance of three feet, even when the needle is not very moveable. I have not observed greater effects from a Galvanic apparatus composed of forty similar troughs: indeed, the effect seems less great."

By comparing the discoveries of Œrsted with those detailed in Sir Humphry Davy's first paper, it will be observed that both philosophers were engaged in the same pursuit, and that both developed the same facts, about the same time, without any knowledge of each other's views or occupations; and, what seems still more interesting, is their dissimilar modes of investigation. The English philosopher estimated the electro-magnetic powers of the conducting wire by the quantity of ferruginous particles that it would lift up; whilst the Danish philosopher made similar calculations by the extent of deflection of a magnetic needle.

In Professor Œrsted's second series of investigations, above alluded to, there is described a very ingenious contrivance for showing the deflection of the conducting wire of a Voltaic apparatus, when approached by a magnet. The apparatus consists of a small rectangular copper trough, with a plate of zinc within it. The trough is three inches high, four inches long, and half an inch broad; the plates are as thin as could be procured, and connected by a thin wire; the exciting liquid is placed in the trough, and the whole suspended by means of an exceedingly thin wire. Fig. 1, Plate A, represents a vertical section of the apparatus across its breadth. The copper trough is shown by *c c c c*, and the zinc plate by z z: *c f f f' f' z* is the connecting wire: *c a c*, a loop of linen thread uniting the apparatus to the wire of suspension, *a b*. When the pole of a bar magnet is presented to either of the arms *f f* or *f' f'* of the conducting wire, the latter is immediately deflected, the whole apparatus turning on the prolonged axis of *a b*.

A variation of this apparatus was made by its author, by coiling the Galvanic plates, within one another, into the shape of a vertical spiral, and suspending them in a vessel containing the liquid conductor. This apparatus, he says, is more moveable than the other, but more precautions are necessary not to be deceived when experiments are made with it.

The structure of the small apparatus, described by Professor Œrsted, has been varied in several ways; MM. Naef and De la Rive, of Geneva; Van der Boss, of Utrecht; and the late ingenious Mr. James Marsh, of Woolwich, have each given it a different fashion. The contrivances of the first two named philosophers were described in the *Bibliotheque Universelle*, for March, 1821, and are the first in point of time; and, as the apparatus of De la Rive is the most interesting of the whole, because of its having developed a principle in Electro-Magnetism not previously known, it is the most deserving of notice in this place. The apparatus consists of a narrow slip of thin sheet copper, and another of zinc. The copper is twice as long as the zinc, in order that it may be bent in the middle, so as to present its two portions to the two surfaces of the zinc, which is placed between them, upon the principle of the Wollaston battery. The metals are connected by a thin wire, formed into an open coil, and fitted to a flat cylindrical cork, in such a manner that when the apparatus is

floating on the liquid conductor, the metals may be wholly immersed, and the plane of the conducting wire-coil stand vertical above the centre of the upper side of the cork. A vertical section of this apparatus, as improved by Mr. Marsh, is represented by Fig. 6, Plate A, in which *c c* is a flat cylinder of cork, *h* the flat open spiral wire which connects the central strip of zinc with the copper that passes round its lower end. The cork has an opening through its centre, for the reception of the open end of a narrow cylindrical glass vessel, which is permanently fixed in the cork by means of cement. The Voltaic pair and the liquid conductor (dilute nitric acid,) are placed in the glass vessel, and the whole floated on a basin of water. The glass vessel holding all the acid liquor that is necessary for an experiment, it is much less expensive than by the method of De la Rive; and much of the resistance to its motions, occasioned by the flat plates, is done away with by the shape and smoothness of the glass vessel.

When the apparatus is prepared for experiment, and floating on water, it is very curiously affected on the approach of a magnetic pole to the coil. When the pole of a bar magnet is presented to the plane of the coil, the latter will either sail on to the bar, or will be repelled from it; but, in the latter case, it will turn half round, approach the magnet, and sail on to it, until it gets to about its middle point, which appears to be its resting place. These curious motions are occasioned by the magnetic polarity of the coil, presenting north polarity on one side and south polarity on the other; so that its approaching the magnet, or receding from it when first presented, will depend upon the *kind* of pole that approaches it, but its constant tendency to place itself on the equator of the magnet, developed a novel feature in Electro-Magnetism.

Fig 5, Plate A, is another form of apparatus, invented by M. Van der Boss. It consists of a small pair of copper and zinc plates, c and z, connected together by a long helix of thin brass wire, *a b*, coilled upon a reed. The plates are prevented touching each other by means of a piece of wood, and are suspended in acid liquor by means of a slender thread, *t*; the ends, *a b*, of the spiral display magnetic polarity with the precision of a magnetized steel bar. This apparatus was first made known in a letter, dated Utrecht, December 8th, 1821, from the late Professor Moll to Dr. Brewster, which appeared in the *Edinburgh Philosophical Journal*, for April, 1822.

In a very early part of the year 1821, Professors Scweiger, of Halle, and Cumming, of Cambridge, employed spiral conducting wires for the purpose of increasing the deflections of a magnetic needle. The plan of the former philosopher was simply that of winding a copper wire several times round a compass box, in the direction of the needle's length when at rest; by which means the electro-magnetic effects are augmented. To this apparatus its author has given the name of *Multiplier:* it was first described in the *Bibliotheque Universelle*, for March, 1821.

The apparatus of Professor Cumming is called a *Galvanometer*. It is an instrument very extensively employed in Electro-Magnetic researches. By means of his Galvanometer, the Cambridge Professor was enabled to compare the electro-magnetic effects of different Voltaic combinations of metals, and acid, and alkaline solutions; and thus to discover several novel facts, and advance our knowledge in this particular branch of physics. Fig. 15, Plate A, which is a spiral of copper wire, with a magnetic needle, *n s*, within it, shows the principle of the Galvanometer: the extremities, *c* and *z*, being connected with a Voltaic battery, the current through the spiral deflects the needle from its first position.

Dr. Faraday's researches in this department of science were first made known in a paper, dated Sept. 11th, 1821, which appeared in the *Quarterly Journal of Science* for the following month. In this paper, some very ingenious contrivances are described, by means of which this excellent experimentalist developed a perfectly novel and distinct class of Electro-Magnetic phenomena. If, for instance, a portion of the connecting wire of a Voltaic battery be so arranged as to be suspended in nearly a vertical position, by means of a universal hinge at its upper extremity, whilst its lower extremity floats in a small shallow dish of mercury—also in the Voltaic circuit—its lower end will perform a revolution in the mercury, around the pole of a bar magnet, placed beneath the centre of the dish; or, rather, round a point in the mercury *above* the magnetic pole. The experiment has a much better effect when the magnet is placed in a vertical position, and its upper end passed through the centre of the dish and rises a little above the surface of the mercury: by these means the wire revolves round the superior pole of the magnet—the direction of motion depending upon the *direction* of the electric current in the wire, and the *kind* of magnetic pole around which it travels.

The converse of the above described action was shown by Dr. Faraday, by producing a revolution of the magnetic pole round a vertical portion of the connecting wire. For this purpose, the magnet had one of its ends attached, by means of a silken thread, to the bottom of the cavity of a deep cup, sufficiently filled with mercury to float the magnet, allowing its upper end to appear above the surface, the vertical wire having its lower extremity immersed in the centre of the fluid metal. When the electric current from the battery was transmitted through the wire and the mercury, the upper end of the magnet revolved round the vertical wire; and, when both magnet and wire were free to move, they performed revolutions in the mercury around each other.

Dr. Faraday contrived a very elegant piece of apparatus, by means of which the revolutions of the wire round the magnet, and the magnet round the wire, may be exhibited at the same time, and by one and the same electric current; one of the movements being performed in one portion of mercury and the other in another portion—the magnets employed being about the size of the barrel of a goose quill.

Fig. 14, Plate A, which is a representation of a section of this neat apparatus, consists of two glass vessels, G and H, a baseboard for them to stand on, and the following appendages: both vessels are open at the bottom, for the purpose of introducing exterior conducting wires to the mercury within, the latter being represented by the shaded parts inside the vessels. The bent wires, C S and Z S, pass through the bottom of their respective vessels, and are amalgamated at their upper ends, so that they may unite with the mercury in the vessels. Two small cylindrical magnets are repersented by *n s* and *n' s'*; that in the vessel H stands vertically in the centre of the mercury, with its upper end a little above the surface of it. It is held firmly in that position by a copper socket, fixed in the narrow part of the glass vessel, the socket being connected to the bent wire, C S. In the vessel, G, the lower end of the magnet is held down by means of a short piece of thread, which keeps the upper part floating obliquely in the mercury.

The pendent wire, L *m*, is suspended from the point L of the stout wire, O E F L, in such a manner that it can move freely round the magnet. On the other side of the apparatus, the thin wire, K, is fixed to the stout wire, O E F L, having its lower extremity immersed in the centre of the mercury. The bent wire is sustained by the brass pillar, A B. When the battery connections are made at C and Z, the current passes through both masses of mercury, and also through the wires that enter their surfaces; and the moveable wire rotates round the fixed magnet, whilst the magnet in the other vessel rotates round the fixed wire. The directions of these motions will depend upon the direction of the electric current and the poles of the magnets exposed to its influence.

Fig. 15, Plate A, represents another exceedingly neat apparatus, invented by Dr. Faraday, for the purpose of showing the rotation of a conducting wire round the pole of a *temporary* magnet. The temporary magnet, in this case, is a small cylindrical piece of soft iron, N S, which is fixed, by means of a cork, in the lower end of a glass tube, and surrounded by mercury. When the pendent moveable wire, *w w*, is made the channel of an electric current from a Voltaic battery, and the piece of iron rendered magnetic by the approach of the pole of a bar magnet to its inferior extremity, the wire revolves round the upper end of the iron. If, now, the pole of the magnet be changed, the polarity of the iron becomes changed in accordance, and the rotation of the wire will also be reversed. Or, on the other hand, if the piece of iron be permitted to retain its first polarity, a reversal of the direction of the electric current will be productive of a reverse direction of motion in the pendent wire.

The same ingenious philosopher, by reversing the experiment of De la Rive, instituted another no less instructive. In this case, a portion of the connecting wire of a battery was formed into a long open spiral or helix, and held, with its axis horizontal, in a basin of water, near to a magnetic needle, floating by means of a morsel of cork.

When the axis of the spiral was placed in the same horizontal line with the axis of the magnetized needle, the latter, if its proper pole were nearest, would sail towards the spiral, enter it, and settle in the middle of its interior. If, however, the wrong pole of the magnetic needle were presented to the helix, it would recede from the latter by a mutual repulsion of the two, change its direction, and enter the helix as before stated. If the needle were forcibly introduced to the interior of the helix, with its poles in the opposite direction to that in which it would be retained, it would be immediately shot out again, or ejected, as soon as the hand which introduced it was removed. The apparatus for showing this beautiful and interesting experiment is represented by Fig. 7, Plate A.

In a subsequent paper, which appeared in the January number of the *Quarterly Journal of Science*, for 1822, Dr. Faraday described a method of rotating a moveable portion of the connecting wire by the action of the earth's Magnetism, without the aid of any artificial magnet whatever. These beautiful experiments of Dr. Faraday, as soon as they were generally known, became the objects of general attention, and were studied with great interest.

About this time an ingenious little instrument was invented by M. Ampere, for the purpose of illustrating the rotations of a conducting wire round the pole of a magnet, independently of a large Voltaic battery. The apparatus consists of an annular copper vessel, about two inches in height, and three inches diameter, made of two concentric cylinders of copper, joined together at one end by means of an annular disc of the same metal, which serves as a bottom to the vessel. Through the central opening of the vessel passes one end of a cylinder bar magnet, kept in a vertical position by means of a wooden foot or stand. The copper vessel hangs on the top of the magnet, by means of an arched handle or bridge of stout wire, which reaches across the top of the central opening—its two ends being soldered to the two opposite edges of the inner copper cylinder. Upon the top of this arched connecting wire is placed the pivot of another arched wire, to the dependent extremities of which is soldered a hollow cylinder, or hoop, of sheet zinc, which hangs freely in the copper vessel, without touching its sides. The copper vessel, with the zinc cylinder, form a Voltaic apparatus; and, when acid liquor is poured into the former, so as to reach the zinc, the latter, with its arched conductor, perform a revolution around the magnetic pole; and, by reversing the position of the magnet, the direction of motion will be the reverse of the former.

Mr. James Marsh improved this apparatus, by introducing a pivot to the centre of the arched wire of the copper vessel, by means of which the whole of the apparatus is at liberty to rotate on the pole of the magnet—the zinc moving in one direction, and the copper in the other, forming a very pleasing compound motion. Fig. 16, Plate A, is a representation of this neat Voltaic apparatus.

M. Ampere also succeeded in rotating a small magnet on its axis, by the influence of an electric current, directed through it from one of its poles to nearly its centre. Fig. 11, Plate A, represents a vertical section of the cylindrical magnet, of the exact size first employed in this experiment: N S is the steel magnet, and *c p* represents a cylindrical piece of platinum, screwed into the lower end of the magnet at *c*, for the purpose of keeping the whole in a vertical position when floating in mercury. Another screw hole is made at the upper end of the magnet, in order that the position of its poles may be reversed in the mercury, when the platinum is attached to the end, N.

Fig. 10, shows the arrangement of the apparatus when ready for experiment: N S is the small magnet floating vertically in a portion of mercury contained in a glass vessel; P is the platinum weight which sinks that end of the magnet to which it is attached, so as to allow a sufficient portion of the upper end to appear above the surface of the mercury. C E O N and Z F N are two conducting wires; the former is bent at E and O, and finely pointed at N, where it dips into a globule of mercury placed in the screw hole. To the extremity, N, of the wire, Z F N, is attached a hollow cylinder, or hoop, of thin sheet copper, which enters the glass vessel, so that its lower edge comes into contact with the surface of the mercury. When the battery is brought into connection with these conducting wires at the extremities, C and Z, the current is transmitted from C to E, O, and N, where it enters the upper end of the magnet, and proceeds through it to the surface of the mercury: it then enters the copper hoop, which, together with the wire F Z, conveys it to the other end of the battery. By these means the magnet is forced into a rotatory motion on its axis. The direction of motion is reversed either by merely changing the direction of the electric current, or by reversing the position of the magnet without any change in the direction of the current. Fig 10, is merely a representation of the arrangement of the connecting wires, the magnet, and the mercury, independently of any other part of the apparatus; which, when complete with its mahogany base-board and cups for holding mercury, for convenience of connecting its different parts with the battery, has a very elegant appearance. The apparatus is simplified by dispensing with the platinum weight, and having the lower end of the magnet furnished with a fine steel point, which runs in an agate cup, fixed at the bottom of the cavity of the vessel that holds the mercury. The copper hoop has also been done away with, by the insertion of a conducting wire through the side of the vessel, having its inner end in connection with the mercury. I believe these improvements are due to the late Mr. James Marsh.

In a second series of Electro-Magnetic researches, by Sir H. Davy, which appeared in the *Philosophical Transactions*, for 1821, we find that this excellent philosopher occasionally employed the most extensive Voltaic battery hitherto constructed; by means of which he was enabled to develop a highly interesting phenomenon, which he had failed in producing in previous trials, whilst using a comparatively feeble

Voltaic force. The battery in question belonged to the London Institution, and consisted "of 2000 double plates, of zinc and copper, with a mixture of 1168 parts of water, 108 parts of nitrous acid, and 25 parts of sulphuric acid: the poles were connected by charcoal, so as to make an arc, or column, of electrical light, varying in length from one to four inches, according to the state of rarefaction of the atmosphere in which it was produced; and, a powerful magnet being presented to this arc, or column, having its pole at a very acute angle to it, the arc, or column, was attracted or repelled with a rotatory motion, or made to revolve by placing the poles in different positions, according to the same law as the electrified cylinders of platinum described in my last paper, being repelled, when the negative pole was to the right hand, by the north pole of the magnet, and attracted by the south pole, and *vice versa*.

"It was proved by several experiments that the motion depended entirely upon the Magnetism, and not upon the electrical inductive power of the magnet, for masses of soft iron, or of other metals, produced no effect.

"The electrical arc, or column of flame, was more easily affected by the magnet, and its motion was more rapid when it passed through dense than through rarified air; and, in this case, the conducting medium, or chain of aeriform particles, was much shorter."

Besides this interesting fact, which shows that, independently of any conducting wire, or solid conductor whatever, an electric current, transmitted through air and in the shape of a brilliant flame, is affected by the influence of external magnetic forces, Sir Humphry relates many other important results which he arrived at in this series of researches—most of which were directed to the relative conducting power of various kinds of metal at different temperatures. The two following paragraphs will afford an idea of the nature of the experiments, and of the inferences which our philosopher arrived at:—

"The most remarkable general result that I obtained by these researches, and which I shall mention first, as it influences all the others, was, that *the conducting power of metallic bodies varied with the temperature, and was lower in some inverse ratio as the temperature was higher.*

"Thus, a wire of platinum of 1-220, and three inches in length, when kept cool by oil, discharged the Electricity of two batteries, or of twenty double plates; but, when suffered to be heated by exposure in the air, it barely discharged one battery. Whether the heat was occasioned by the Electricity, or applied to it from some other source, the effect was the same."

Another series of exceedingly interesting phenomena was discovered by the researches of Sir H. Davy, by transmitting powerful electric currents through large masses of mercury. They are described in a paper by the author, which appeared in the *Philosophical Transactions*, for 1823. The battery employed belonged to the

London Institution, and consisted of a pair of copper and zinc plates, of about 200 square feet. The following extracts are descriptive of the reasoning which led to the experiments, and of the plan adopted for their execution:—

"Immediately after Mr. Faraday had published his ingenious experiments on Electro-Magnetic Rotation, I was induced to try the action of a magnet on mercury connected in the electrical circuit, hoping that, in this case, as there was no mechanical suspension of the conductor, the appearances would be exhibited in their most simple form; and I found that when two wires were placed in a basin of mercury, perpendicular to the surface, and in the Voltaic circuit of a battery with large plates, and the pole of a powerful magnet held either above or below the wires, the mercury immediately began to revolve round the wire as an axis, according to the common circumstances of Electro-Magnetic Rotation, and with a velocity exceedingly increased when the *opposite* poles of two magnets were used—one above, the other below.

"Masses of mercury, of several inches in diameter, were set in motion, and made to revolve in this manner, whenever the pole of the magnet was held near the perpendicular of the wire; but, when the pole was held above the mercury, between the two wires, the circular motion ceased, and currents took place in the mercury in opposite directions, one to the right and the other to the left of the magnet. These circumstances, and various others which it would be tedious to detail, induced me to believe that the passage of the Electricity through the mercury produced motions independently of the action of the magnet, and that the appearances which I have described were owing to a composition of forces.

"I endeavoured to ascertain the existence of these motions in the mercury, by covering its surface with weak acids, and diffusing over it finely divided matter, such as the seeds of lycopodium, white oxide of mercury, &c. but without any distinct result. Then it occurred to me that, from the position of the wires, currents, if they existed, must occur chiefly in the lower and not the upper surface of the mercury, and I consequently inverted the form of the experiment. I had two copper wires, of about one-sixth of an inch in diameter, the extremities of which were flat and carefully polished, passed through two holes, three inches apart, in the bottom of a glass basin, and perpendicular to it; they were cemented into the basin, and made non-conductors by sealing-wax, except at their polished ends; the basin was then filled with mercury, which stood about a tenth or a twefth of an inch above the wires. The wires were now placed in a powerful Voltaic circuit: the moment the contacts were made, *the phenomenon*, which is the principal object of this paper, occurred: the mercury was immediately seen in violent agitation; its surface became elevated into a small cone above each of the wires; waves flowed off in all directions from these cones, and the only point of rest was apparently where they met in the centre of the mercury, between the two wires. On holding the pole of a powerful bar magnet at a considerable

distance (some inches) above one of the cones, its apex was diminished and its base extended; by lowering the pole further, these effects were still further increased, and the undulations were feebler. At a smaller distance, the surface of the mercury became plane, and rotation slowly began round the wire. As the magnet approached, the rotation became more rapid; and, when it was about half an inch above the mercury, a great depression of it was observed above the wire and the vortex, which reached almost to the surface of the wire."

In some of Sir Humphry's experiments, fused tin was used in place of mercury, and steel wires were substituted for those of copper; but, with the exception of degree, the effects were still the same.

In an early part of the year 1823, Mr. Barlow, of the Royal Military Academy, at Woolwich, published the second edition of his *Magnetic Attractions*, in which a section is devoted to Electro-Magnetism, containing novel investigations, and several new experiments, partly by himself and partly by Mr. James Marsh. The latter gentleman discovered that a pendulous portion of a conducting wire, carrying an electric current, and placed between the poles of a horse-shoe magnet, would perform a singular sort of a vibratory motion, which will be easily understood by referring to Fig. 12, Plate A, which is a perspective view of the apparatus eventually employed for exhibiting the phenomenon.

A B Is a rectangular mahogany base-board, from which rises a brass pillar, P M, which is hollow from M downwards to the distance of about an inch, for the introduction of the thin bent wire, *o z* M, which, when properly adjusted, is fixed in its place by means of the set-screw, M. From *o* the penulous wire, *o s*, hangs, having its lower end immersed in a small portion of mercury, placed in a grove at *c*. A horse-shoe magnet, *s e n*, is laid on the base-board, with the pendulous wire and mercury between its poles. If, now, an electric current from a Voltaic battery be transmitted through the pendulous wire, its lower end will be immediately thrown out of the mercury in a plane perpendicular to a line joining the magnetic poles: an interruption is thus formed in the electrical circuit, and the wire falls again to its former position; but it no sooner touches the mercury than the current is resumed, and the wire projected as before; and thus, by a series of openings and closings of the circuit, the wire is kept in a state of vibration. The battery connections are made at M, and in a small channel of mercury, joining the mass at *c*. The direction in which the wire will be projected from the mercury depends upon the direction of the current and the position of the magnetic poles: so that it may be thrown into the position of either of the dotted lines, according to these conditions.

The vibrating wire of Mr. Marsh furnished Mr. Barlow with the idea of forming a stellate wheel, which rotated on a horizontal axis between the poles of a horse-shoe magnet. Fig. 13, Plate A, represents the apparatus employed in this experiment.

A B is a rectangular board, from which rises the wooden pillar, C D, which has a socket at the upper end for the reception of the bent wire, D E F, which is kept firmly in its place by means of the set-screw at D. From F descend two brass arms, with pivot-holes at their extremities for the reception of the finely-pointed extremities of the axle of the stellate wheel, the lowest points of which dip into mercury, placed in a cavity made in the base-board. The horse-shoe magnet, *n e s*, is laid on the board, so that the lower edge of the wheel may be embraced by its poles. When the battery connections are made at E and *i*, the current will run through the lower part of the wheel between the axle and the mercury; and those tips of the wheel which touch the mercury will be thrown out, and the next in the series will succeed them,—and thus, by a succession of impulses, upon the same principle as that which projects Marsh's vibrating wire, the wheel is kept revolving with great velocity. The direction of motion will depend upon the direction of the electric current and the position of the magnetic poles: hence a reversal of either will reverse the direction of motion in the wheel. Mr. Barlow varied the wheel apparatus by fixing two stellate wheels on the same axle; which, by a proper application of electric currents, and a magnet to each wheel, rotated with greater rapidity than the single wheel.

Another piece of apparatus, contrived by Mr. Barlow, consisted of a thin brass cylinder, closed at one end and open at the other. The inside of the closed end was furnished with a finely-pointed pivot, on which it could rotate on the pole of a vertical bar magnet, which passed up the interior of the cylinder, the lower end of which terminated in an annular wooden vessel containing mercury, in which it could move without much resistance. Fig. 17, Plate A, is a sectional representation of this neat apparatus. N S is a portion of the magnet, on which is fixed the annular vessel, A A' B B', containing mercury, *a d*, in the circular cavity. The part *a b c d* represents the hollow brass cylinder with its pivot at *f*, which works in a small cavity on the top of the magnet. Just over the pivot on the outside of the dome of the cylinder is a small cup, *e*, for holding a globule of mercury, in which is immersed the finely-pointed termination, Z, of the bent conducting wire, A Z. Through the side of the wooden annular vessel, at A, passes one end of a bent wire, furnished with a cup, C, at the other end, for holding mercury. The inner end of this wire is amalgamated, and in contact with the annular mass of mercury. When the battery contacts are made at Z and C, the electric current is transmitted from one to the other through the suspended cylinder, which rotates on its pivot by the influence of the interior magnet, on the same principle as the rotation of the wire in Dr. Faraday's experiment—the cylinder being considered as a united assemblage of vertical wires, every one of which carries a portion of the current. This is the explanation given by Mr. Barlow, who also informs his readers that he was led to institute the experiment from the hints furnished by Sir H. Davy's mercurial vortices, already described.

In the *Annales de Chimie et de Physique*, for 1823, M. Becquerel described a Galvanometer of a peculiar structure and great sensibility, by means of which this eminent philosopher was enabled to detect the most feeble electric currents. The apparatus consists of three spiral wires, each containing a magnetic needle, delicately suspended, by means of a fibre of silk from the cocoon, and so arranged that the north pole of one is within the sphere of action of the south pole of the next in the series; so that they assist one another's deflections, whilst an electric current is passing through the spirals of the conducting wire.

Besides the philosophers already noticed, several others, in different countries, had entered on Electro-Magnetic inquiries, previously to the close of the year 1823, amongst whom were the late illustrious Berzelius, of Stockholme; Professors Poggendorff, Erman, and Seebeck, of Berlin; MM. Van Beek and Van Rees, of Liege; Professor Moll and M. Van Buck, of Utrecht; MM. Biot, Gay Lussac, and Savart, of Paris; MM. Antinori, Ridolfi, and Gazzeri, of Florence; Dr. Wollaston, and some others, of this country. But the facts already described are nearly all that were developed up to that period, with the exception of such as were strictly applicable to mathematical investigations of the laws of Electro-Magnetic forces—the most interesting of which were by MM. Ampere, Biot, Demonferrand and Savart, of France; Professor Barlow, of Woolwich; and Dr. L. F. Kaemtz, of Halle; all of which are exceedingly interesting, but not essential to this sketch.

The theoretical views that were taken respecting the *modus operandi* in the production of Electro-Magnetism were various, as might be expected, in the infant state of the science. Professor Œrsted's views are founded on the supposition of the existence of two electric fluids, each of which has a peculiarity of action, and that both are simultaneously brought into play in every electric discharge, whether from a Leyden jar or from a Voltaic apparatus. These electric fluids, which the Professor calls the *positive* and the *negative*, taking opposite directions in the conducting wire, consequently meet with one another, and are thus supposed to have a rencounter, or to produce an *electrical conflict*,* which acts, *magnetically*, both within the conducting wire, and around it in space to a considerable distance.† The *electrical conflict* is supposed to act in circles whose planes are perpendicular to the axis of the conducting wire; "all of which," says our author, "are easily understood by supposing that the *negative* Electricity moves in a spiral line bent towards the right, and propels the north pole, but does not act upon the south pole. The effects on the south pole are explained in a similar manner, if we ascribe to positive Electricity a contrary motion, and a power of acting on the south pole but not on the north pole."‡

In a subsequent paper, which appeared in Thompson's *Annals of Philosophy*, for November, 1821, Professor Œrsted has given a modification of the above hypothesis

* See page 9. † See page 11. ‡ See page 11.

in the following words:—" When opposite electrical powers meet under circumstances which offer resistance, they are subjected to a new form of action, and in this state they act upon the magnetic needle in such a manner that positive Electricity *repels* the south and *attracts* the north pole of the compass, and negative Electricity *repels* the north and *attracts* the south pole; but the direction followed by the electrical powers in this state is not a *right line*, but a spiral one, turning from the left hand to the right." It is obvious that this second edition of Œrsted's hypothesis differs from the former in nothing more than that of allowing each of the two supposed spiral electrical powers to operate on *both* poles of the needle, instead of on one pole only.

The theoretical views of M. Ampere have already been stated in his paper, dated September 25th, 1820.* This eminent philosopher admits also of two electric currents, which flow in opposite directions in the conducting wire, and are productive of magnetic action. He also supposes that *all* magnetic forces, even those displayed by bars of steel, are due to similar electric currents—an hypotheses which derives no support either from fact or analogy.

Dr. Wollaston supposed that " the phenomena exhibited by the electro-magnetic, or conjunctive wire, might be explained upon the supposition of an electro-magnetic current playing round the axis of the conjunctive wire, its direction depending upon that of the electric current, or upon the poles of the battery with which it is connected."†

The Marquis Ridolfi supposed that, because Electricity produces both magnetic and calorific phenomena, the elements producing these separately may possibly be so compounded together as to produce Electricity; which infers that Electricity is a compound of Magnetism and caloric.‡

In Thompson's *Annals of Philosophy*, for July, 1822, there is an attempt made to explain the manner in which the connecting wire acts on the needle, by M. Prechtel, Director of the Polytechnic Institution in Vienna; but, his diagrams and mode of reasoning appearing too complex for admission to this sketch, the reader is referred to the original article for further information respecting M. Prechtel's hypothesis.

Whilst prefacing his investigations respecting the action that takes place between a connecting wire and magnetic needle, Professor Barlow remarks:—" All the experiments that have been made on the subject of Electro-Magnetism, since the first discovery of that power by M. Œrsted, seem to indicate a *strong affinity*, although not a *complete identity* between the simple magnet and the *electro-magnetic* fluid; or if the identity be admitted, still a certain difference must be conceived to have place in the modes of action." And again—

" I have been led to consider that all the apparently anomalous effects produced on a magnetized needle, by the action of a Galvanic wire, might be explained by the

* See page 12. † Quarterly Journal of Science, for January, 1821. ‡ Bibliotheque Universelle, for February, 1821.

admission of one simple principle: viz. that every particle of the Galvanic fluid in the conducting wire acts on every particle of the magnetic fluid in a magnetized needle, with a force varying inversely as the square of the distance; but that the action of the particles of the fluid in the wire is neither to attract nor repel either pole of a magnetic particle, but a *tangential force*, which has a tendency to place the poles of either of the fluids at right angles to those of the other; whereby a magnetic particle, supposing it under the influence of the wire only, would always place itself at right angles to the line let fall from it perpendicular to the wire, and to the direction of the wire itself at that point."

"I pretend not," says Professor Barlow, "to illustrate the mechanical principles by which such an action can be produced; I propose only to show that, if such a force be admitted, all the results obtained from the reciprocal action of a Galvanic wire and magnetized needle may not only be explained, but computed; and that the results agree numerically with experiment."*

THERMO-MAGNETISM.†

The first public announcement, in this country, of this simple mode of developing Magnetic action, was in the October number of Thompson's *Annals of Philosophy*, for the year 1822, in the following words:—

"M. Nordenskiold, of Abo, now in this country, has made known the following curious experiment of Dr. Seebeck, of Berlin. Take a bar of antimony, about eight inches long and half an inch square; connect its extremities by twisting a piece of brass wire round them, so as to form a loop, each end of the bar having several coils of wire. If one of the extremities be heated for a short time with a spirit lamp, Electro-Magnetic phenomena may be exhibited in every part of it." It subsequently became known that Seebeck had carried on an extensive series of experiments on this subject.

Professor Cumming, of Cambridge, appears to have been the first English philosopher who entered upon investigations in this novel branch of research. His experiments, which were both numerous and highly interesting, were described in a memoir which was read before the Cambridge Philosophical Society, April 28th, 1823. In these investigations, Professor Cumming experienced a great convenience in the employment of his *Galvanometer*, an exceedingly useful apparatus for the detection of

* Barlow's Magnetic Attractions, Second Edition.

† The phenomena that come under this head are sometimes called the Thermo-Electric, although allowed to be Electro-Magnetic.

feeble electric currents, and for ascertaining the directions in which they flow—qualifications which confer upon it a high value, and render it almost indispensible in these nice and delicate experimental inquiries. The experiments of this philosopher extended through a great variety of metallic combinations, both in the shape of bars and wires, and also on small morsels of metal, which enabled him to classify their electrical relations in giving birth to Electro-Magnetic phenomena, by a mere change of temperature in some particular part of each individual arrangement. Several interesting tables, exhibiting at one view the results of a great number of experiments, are attached to the Memoir.*

About the same time that Professor Cumming was making these experimental inquiries at Cambridge, the Dutch Philosophers, Dr. Van Beek, Major-General Baron Van Zuylen, Van Nyevelt, and Professor Moll, were carrying on similar investigations at Utrecht. Having no Galvanometer, their experiments were conducted in a very different manner to the generality of those of Professor Cumming, their apparatus differing very little, in point of shape, to that first employed by Dr. Seebeck. They, however, operated upon various metals both separate and combined, and obtained some very interesting results, as far as the deflections of a magnetic needle are concerned. Their modes of accomplishing a difference of temperature in the metallic combinations employed were various: sometimes by means of a spirit lamp, and at others by hot water; and, in some experiments, a depression of temperature was obtained by the local application of a piece of ice.†

Thermo-Magnetism was also studied on the continent by M.M. Baron Fourier, Œrsted, De la Borne, Becquerel, Demonferrand, and some others; but the extent of their inquiries was much within the limits of those of Seebeck, or of the Cambridge Professor.‡

In the *Philosophical Magazine*, for November, 1823, Professor Barlow published a paper, descriptive of a series of Thermo-Magnetic experiments, conducted principally by Mr. James Marsh. The apparatus employed consisted of rectangular combinations of platinum and silver wires, furnished with pointed pivots, on which they could turn in a horizontal plane; and, in some cases, they would perform rotations when excited by the flames of a spirit lamp, and placed under the influence of a magnetic pole, situated *exterior* to one of the ends of any individual rectangle; for, as the electric current *ascended* in one of the ends and *descended* in the other, a magnetic pole, placed between them in the centre of motion, could not effect a rotatory motion, because of the tendencies to motion of the two parts of the current in the two ends of the rectangle being opposite to each other: hence any rotations that were produced by these combinations were the effects of a succession of distinct impulses upon the

* Transactions of the Cambridge Phil. Society, vol. 2. † Edinburgh Philosophical Journal, for July, 1823.
‡ Annales de Chimie et Physique, for 1823.

ends of the rectangle, as they respectively entered the flame of the lamp, thus producing a new current, and came within range of the magnetic force emanating from the steel bar.

In concluding this brief sketch, it is but due to the merits of the late ingenious Mr. James Marsh to state, that, besides the invention of some electro-magnetic apparatus, he constructed a very compact set for exhibiting nearly all the experiments that had been made public till the close of 1823, and, on presenting these apparatus to the Society of Arts, he was honoured with their large silver medal and a purse of thirty guineas.

SECTION II.

AN ABSTRACT OF SCIENTIFIC DISCOVERIES, INVENTIONS, &c. BY WILLIAM STURGEON. CHRONOLOGICALLY ARRANGED ACCORDING TO THE ORDER OF THEIR RESPECTIVE DATES.

In the preceding *Historical Sketch*, every attention has been paid to the dates of discoveries, and to place them to the credit of those philosophers to whom they are due, at least, as far as was possible by collecting the materials from the Scientific Journals in which the several facts originally appeared. In some cases, however, similar discoveries were made at nearly the same time, in different countries, without any understanding between the parties who made them. Under these circumstances, the credit is obviously alike due to each individual philosopher engaged in developing the phenomena. But there are other cases in which certain discoveries have been made a second and even a third time, by different parties, at very different periods. In those cases, although there can appear no doubt respecting a genuineness of originality in the whole, the credit of discovery has invariably been placed to the account of those philosophers who first announced the development of the facts. But, notwithstanding the advertency that has been bestowed in collating the several events, with their attendant circumstances, and the desire of doing justice to all parties, it is still possible that some slight errors, that have escaped detection, may have crept in.

Although, during this period, I had myself paid considerable attention to the subject of Electro-Magnetism, I am not aware that any of my labours were made public, through the medium of Scientific Journals, till September, 1823: it is, therefore, from that period that my earliest researches in this branch of physics are to be

dated, although it is obvious that discoveries and inventions must have an existence previously to their announcement in any public journal, and, in many cases, the difference in point of time is very considerable.

1st. The first piece of my apparatus that became known to the scientific world is a modification of Ampere's rotating cylinders, as improved by Mr. Marsh. The first apparatus of this kind was disposed of to Mr. Jones, Optician, Holborn, London, who purchased it to send to the United States of America. Mr. Jones was kind enough to draw up the following short account of it, which he got introduced to the pages of the *London Philosophical Magazine*, for September, 1823:—

"The apparatus consists of two sets of revolving cylinders, one suspended on each pole of an inverted horse-shoe magnet. Upon the usual insertion of diluted nitric acid, the two sets of cylinders simultaneously enter into rotations in a very interesting and striking manner. This form of the magnet gives increased power on a reduced altitude, and the proximity of the poles materially augments the rotation of the opposed cylinders. The effect is the most pleasing we have ever seen, and was witnessed at the house of Messrs. Jones, Opticians, Holborn."

Prior to this apparatus making its appearance, a straight bar magnet had invariably been employed to show the action of Ampere's cylinders; the rotations were first performed on one pole, and afterwards the magnet had to be inverted and the cylinders again mounted on it before the rotations on the other pole could be accomplished. By this tedious process a considerable portion of the Galvanic power of the cylinders was wasted, during the time that preparations were making for performing the latter part of the experiment: and, as the rotatory motions are in opposite directions on the north and south poles of the magnet, it required an effort of the memory to recollect the motions exhibited on one pole whilst witnessing those performing on the other. All these inconveniencies are avoided by the apparatus alluded to by Mr. Jones, and represented by Fig. 6, Plate VI. The appearance of this apparatus is much enhanced by the external copper cylinders being cased with polished brass, and by having a brass stand or foot to support the whole, as shown in the figure. Its elegance and performance on the lecture table, will always command for it a conspicuous position.

2nd. *Researches in Electro and Thermo-Magnetism.*

(Originally published in the London Philosophical Magazine, for February, 1824.)

This series of researches was instituted for the purpose of ascertaining the relations that subsist between the electro-magnetic phenomena displayed by the Voltaic and the Thermo-modes of exciting electric currents—the former requiring liquids in the process: hence, sometimes called the *Hydro-Electric* process; and the latter requiring

heat only, which gave rise to the term *Thermo-Electric*. The inquiry led to novel modes of experimenting, and to the construction of several pieces of novel apparatus. The particulars of this inquiry form Part I. of the first of these Memoirs.

3rd. *Researches in Electro and Thermo-Magnetism.*

(Originally published in the London Philosophical Magazine, for April, 1824.)

The principal inquiry in this communication is similar to that in the preceding one, and may be regarded as a continuation of it. Some new methods of experimenting are introduced, with novel apparatus suitable to the inquiry. The particulars will appear in Part II. of the first Memoir.

4th. *Researches in Thermo-Magnetism.*

(Originally published in the London Philosophical Magazine, for April, 1824.)

This communication is descriptive of a novel piece of apparatus, by means of which rotatory motions are produced by thermo-electric currents, under precisely the same circumstances, with respect to central magnetic poles, as the Voltaic-electric currents in Ampere's rotating cylinders (Fig. 16, Plate A) perform their motions around the pole of a magnet. This is the first experiment on record that demonstrated a complete analogy of rotatory powers, or tendencies, in the two classes of electric currents. The particulars form Part III. of the first Memoir.

In addition to the above, a supplement is attached to the first Memoir, containing descriptions of apparatus, which have not, till now, appeared in any scientific work, although as interesting as any of the rest, and in close correspondence with the subjects embraced in the Memoir.

5th. *Researches in Electro-Magnetism.*

(Originally published in the London Philosophical Magazine, for October, 1824.)

The particulars of these researches are embraced in the third Memoir, consisting principally of two novel facts, with descriptions of apparatus invented for their especial exhibition. The first consists of a Galvanic sphere, which performs a revolving motion around a central system of magnets, being operated on by both poles at the same time; and is the first piece of apparatus that demonstrated a concert of action exerted by the north and south magnetic poles, in producing *similar* rotations by electric currents flowing in *similar directions*, with respect to those poles. In all previous rotations, the movements of *similar* currents were performed in directions opposite to each other, when subjected to the influence of dissimilar poles of the

magnet. The second apparatus described in this Memoir shows that a straight bar magnet will rotate on its axis by the joint influence of two electric currents, when flowing at the same time through its north and south regions, in *similar* directions with respect to its poles and equator, viz.:—when both currents flow from the equator to the poles, one in each direction; or, if the currents flow from the poles to the equator, the magnet is forced into a rotation on its axis, but the direction of motion is opposite to that accomplished by the former arrangement.

The description of another elegant piece of apparatus, which has not, till now, appeared in any scientific work, forms a supplement to the third Memoir; to which is added a few remarks on the analogy displayed by the Electro-Magnetic and Astronomical phenomena.

6th. *A complete Set of Novel Electro-Magnetic Apparatus.*

(Originally published in the Transactions of the Society of Arts, for 1825.)

This set of apparatus was presented to the Society of Arts, &c. in May, 1825, for which I was awarded their large silver medal and a purse of thirty guineas.

Previously to the appearance of these apparatus, philosophers had made all their experiments on a very minute scale, although the electrical power employed was formidable, being the production of Voltaic batteries of large dimensions. In the year 1823, the late Mr. James Marsh made a neat set of nearly all the different pieces of Electro-Magnetic apparatus that had been previously invented by various philosophers who had been engaged in cultivating the science, to which he attached a Voltaic battery of about eight square feet of metallic surface, which was the smallest at that time used for carrying on a complete course of Electro-Magnetic experiments; but, although he improved the appearance of some parts of the apparatus, and facilitated the operations with others, they were still left on the original diminutive scale, possessing but feeble magnetic powers, and requiring considerable nicety of adjustment, as well as experimental dexterity, to bring them into successful operation.

The construction of the apparatus alluded to at the head of this article, was undertaken in consequence of having discovered, by previous trials, that the power of the Voltaic battery in Electro-Magnetic processes might be reduced to almost any extent, provided the magnets employed were sufficiently large and powerful. This fortunate discovery enabled me to dispense with nearly all the old apparatus, and to construct an entirely novel set, of much superior magnitude; and, at the same time, to reduce the Voltaic battery to the size of a pint pot, which rendered its management exceedingly easy. A description of these apparatus, with two plates of illustration, is given between the third and fourth Memoir.

7th. *On an apparent Anomaly in the laws of Electro-Magnetic Rotations.*

(Originally published in the London Philosophical Magazine, for June, 1825.)

The announcement of the phenomenon was made to the Editors of the *Philosophical Magazine* in the following manner:—

"Gentlemen,—It appears to have been hitherto remarked by every writer on Electro-Magnetism, that the direction of rotation is always reversed by changing the direction of the Galvanic current, provided the magnetic bar remains unmolested. It has so happened, however, in one of my experiments, whilst rotating a Galvanic wire round the pole of a magnetic bar, that, although the latter be not altered in its position, yet the former will continue to rotate round it in the same direction, whatever change be made with respect to battery contact—that is, whether the wire be descending from the zinc or from the copper side of the battery.

"Now, as this phenomenon appears somewhat anomalous to every other Electro-Magnetic experiment yet made public, I should feel obliged if any of your scientific correspondents would make known, through the medium of your valuable and widely disseminated journal, if a similar phenomenon has ever occurred during their own experiments, or, according to the present doctrine of Electro-Magnetism, under what circumstances this *invariable* rotation can possibly happen.

Yours obediently,

"William Sturgeon.

"Artillery Place, Woolwich, January, 1825.

"*To the Editors of the Philosophical Magazine.*"

It will be observed that the above announcement of a fact (which was occasioned by a novel mode of experimenting,) was made in the shape of a problem to the competitors in Electro-Magnetic discoveries; but, whether from inattention or from whatever cause it is to be attributed, no attempt at explanation made its appearance until I described the whole process of experimenting, in the *Philosophical Magazine*, for March, 1832. The phenomenon, however, is one of those forerunners in experimental science which open new paths of inquiry, and which sometimes lead to the most important results. The fact in question belongs to a distinct class of Electro-Magnetic phenomena, the display of which has no dependence whatever on permanent steel magnets, nor on loadstone, but on the temporary Magnetism of soft iron. It is fully described, amongst others of the same class, in the fourth Memoir.

8th. *An original mode of Investigating the Action of Magnets on Non-Ferruginous Metals.*

(Originally published in the Edinburgh Philosophical Journal, for July, 1825.)

Early in the spring of 1805, some curious and very interesting experiments, by M. Arago, first became known in England. This excellent philosopher had found that

copper and other metallic plates of a non-ferruginous character, when put into rapid rotation beneath a magnetic needle, caused it to deviate from its true position, and, in some cases to rotate in the same direction as the rotating disc. The announcement of this novel fact excited much attention amongst the philosophers of this country; and the experiments were first repeated, with several variations, by MM. Babbage and Herschel, and immediately afterwards by Professors Christie and Barlow, Mr. Marsh and myself. With respect to my own inquiries, however, but little can be said in this place, beyond stating that they led to modes of experimenting very different from those pursued by the above-named philosophers, and eventually conducted me to the brink of Magnetic Electricity—the discovery of which was first announced by Dr. Faraday. The first novel apparatus that I employed in these investigations was described, without my knowledge, by Mr. Barlow, in the *Edinburgh Philosophical Journal*, for July, 1825, in the following manner, which is there called an "Experiment due to Mr. Sturgeon, of Woolwich":—

"A thin copper plate or wheel, about five or six inches in diameter, was suspended very delicately on an axis, and then one side a little weighted, in order to give it a tendency to oscillate. The heaviest point was now raised level with the axis, and the number of vibrations the plate made, before it came to rest, were counted. The same was again done, with this difference only, that the vibrations now took place between the poles of a horse-shoe magnet; and the number of them, before the plate came to rest, was very little more than one-half of what they were in the former instance.

"This is the converse of M. Arago's experiments, in which he shows the effect of copper and other metallic rings, in diminishing the number of oscillations of a magnetic needle.

"If, instead of a horse-shoe magnet, the contrary poles of two bar magnets be used, the effect is the same as before; but, if the poles of the same name, viz. both north or both south, be employed, then the effect is scarcely perceptible. This is an important result, as it shows that the effect is not due to any kind of resisting medium, as was supposed in the first instance."

9th. *Researches on the Ignition of Gunpowder by Electrical Discharges; and on the Transmission of Electricity through Water.*

(Originally published in the London Philosophical Magazine, for June, 1826.)

The ignition of gunpowder by electric discharges was no novel circumstance amongst Electricians, at the time these researches were first made known; but it appeared to be known as a fact only, without any other explanation than that the electric fluid, being supposed to be a fiery element, would, like any other fire, ignite

such combustible bodies as it came in contact with, provided its quantity was sufficiently great. There had, however, been various contrivances employed for the purpose, in some of which the ignition of the powder was a secondary effect. Mr. Boze appears to be the first on the list: he first melted the gunpowder in a spoon, and ignited the vapour by an electric spark. Dr. Watson mixed the powder with inflammable oil or spirits. Mr. Benjamin Wilson pushed fine metallic points into the cartridge, which became sufficiently heated, by a powerful electric discharge, to ignite the gunpowder; and Cavallo and others mixed fine steel filings with the powder, which, like the metallic points of Wilson, became red hot by the discharge, and fired the gunpowder, as a matter of course. It was discovered, ultimately, however, that gunpowder might be ignited by passing electric discharges through water, and other inferior conducting bodies. The theory of this action, with several original experiments, are explained in the nineteenth Memoir.

10th. *Researches on the Ignition of Gunpowder, and other substances, by Electricity. Also, on the Magnetizing Powers of Electric Discharges, when transmitted through different conducting media.*

(Originally published in the London Philosophical Magazine, for January, 1827.)

A portion of these researches is a continuation of the inquiry respecting the circumstances under which electrical discharges will ignite gunpowder. There is also a distinct series of experiments on the ignition of other inflammable bodies; and a portion is devoted to the magnetic action of electrical discharges from a Leyden jar, under various circumstances. The particulars of this series of researches form the twentieth Memoir.

11th. *Description of an Aurora Borealis, observed at Woolwich, on the Evening of September 29th,* 1828.

The particulars appear in Section IV. of this work, which consists wholly of observations on Auroræ Boreales.

12th. *Experimental Researches in Electro-Magnetism, Galvanism, &c.*

(Originally published in the shape of a Pamphlet, in the year 1830.)

In this pamphlet is comprised an extensive series of original experiments, showing that Electro-Magnetic action may be developed and modified by processes which, at that time, were quite novel; with some practical and theoretical observations on the structure and operation of Galvanic batteries, and on the dry electrical column. Also, several original experiments in Electro-Chemistry, with theoretical observations respecting the dissolution of pure metallic bodies in fluid menstrua. These researches will form the fifth Memoir.

13th. *Observations on an Aurora Borealis, seen at Woolwich, on the Night of January 7th*, 1831.

The particulars are stated in Section IV.

14th. *Researches on the Thermo-Magnetism of Homogeneous Bodies.*

(Originally published in the London Philosophical Magazine, for July and August, 1831.)

In these researches extensive series of experiments had to be undertaken before any definite law could be developed. Eventually, however, it was ascertained that electric currents can be developed in any individual mass of pure metal, by a mere disturbance of temperature at some particular point; and that the *direction* of those currents have a decided reference to the point of heat and the crystalline structure of the metal. The particulars of this division of researches were drawn up for publication several months previous to the date of their appearance in the *Philosophical Magazine:* an explanation of the circumstances causing the delay is attached to the second Memoir, which contains all the particulars of this series of researches.

15th. *Researches on Electro-Magnets.*

(Originally published in the London Philosophical Magazine, for March, 1832.)

The results of these researches is the production of a soft iron electro-magnet, which rotates on its axis by means of the same electric currents that give it polarity. Also, some curious facts respecting Electro-Magnets generally: with observations on the loss of power which magnets of hard steel suffer by removing the armature after excitement. See fourth Memoir.

16th. *Researches on the Distribution and Retention of Magnetic Polarity in Metallic Bodies.*

(Originally published in the London Philosophical Magazine, for April and May, 1832.)

This is one of the series of researches which led me to the threshold of Magnetic Electricity. It contains an extensive series of experiments on metals of various kinds, by putting them into motion whilst under the influence of magnets. The results of the inquiry appear in the ninth Memoir.

17th. *Researches on the Distribution of Magnetic Polarity in Metallic Bodies.*

(Originally published in the London Philosophical Magazine, for July, 1832.)

This series of researches is a continuation of that next preceding; and develop many novel facts, the particulars of which form the tenth Memoir.

18th. *On the Theory of Magnetic Electricity.*

(Originally published in the London Philosophical Magazine, for January, March, and May, 1833.)

This is an attempt, and probably the only one hitherto made public, to reduce the phenomena of Magnetic Electricity to a definite code of physical laws, applicable in all cases. It is illustrated with several engravings, and is fully developed in the eleventh Memoir.

In justice to Dr. Faraday, it is proper here to observe, that certain rules for showing " the relation which holds between the magnetic pole, the moving wire or metal, and the direction of the current evolved," is given in his first paper on this subject.

19th. *Researches on the Thermo-Magnetism of single pieces of Metal ; and on Electro-Decompositions of Compound Metallic Solutions.*

(Originally published in the London Philosophical Magazine, for November, 1833.)

The first part of these researches is attached to the second Memoir, and the second part of them to the sixth Memoir—both in the shape of supplements, in which the particulars are fully described.

20th. *A Caution to Electrical Kite Experimenters.*

(Originally published in the London Philosophical Magazine, for November, 1834.)

The particulars of this communication appear in Section V. of this volume, which is wholly devoted to Thunder-storms, Kite Experiments, &c.

21st. *Magnetical Electrical Experiments, made at the Adelade Gallery, London.*

(Originally published in the London Philosophical Magazine, for November, 1834.)

The particulars of these experiments are detailed in Section VI. of this volume, which is devoted to miscellaneous papers.

22nd. *Description of a Thunder Storm, as observed at Woolwich, June 14th,* 1834, *with an Account of an unusual phenomenon, exhibited by means of a kite which was elevated during the storm. Also some Remarks relative to the cause of the Deflection of Clouds from elevated lands.*

(Originally Published in the Philosophical Magazine, for December, 1834.)

For the particulars, see Section V. headed Thunder Storms, &c.

23rd. *A Description of an Aurora Borealis, seen at Woolwich, on the Evening of December 22nd,* 1834.

(Originally published in the London Philosophical Magazine, for April, 1835.)

See Aurora Borealis, Section IV.

24th. *Description of an Aurora Borealis, seen at Woolwich, on the Night of November 16th*, 1835.

(Originally published in the London Philosophical Magazine, for February, 1836.)

See Aurora Borealis, Section IV.

25th. *Researches on the production of Electric Momenta and Shocks by a Voltaic Battery, charged with Water only.*

(Originally published in the London Philosophical Magazine, for August, 1836.)

Several years previously to these researches, the Voltaic battery, when charged with pure water, had been found capable of charging coated glass, to a low intensity, as decidedly as by the action of an electrical machine. The most extensive experiments of this kind were made by Professor Van Marum, of Haarlem, with the extensive battery of jars belonging to the Institution of that place.* But I am not aware of any attempt having been made to produce shocks on the human body, by a water-charged battery, by the same means as these researches have developed: the particulars of which, with some theoretical remarks, form Part I. of the thirteenth Memoir.

26th. *Researches in Electro-Dynamics.*

(Read before the Royal Society of London, June 16th, 1836.)

These researches are devoted to the respective merits of Voltaic Batteries and Magnetic-Electrical Machines, as implements of scientific research, with an extensive series of experiments. The communication also contains descriptions of two distinct, and very differently constructed, Magnetic-Electrical Machines, one of which is perfectly original; also, an original contrivance for *uniting* the *reciprocating* electric currents excited in the revolving coils, and giving them one uniform direction through any apparatus connected with the poles of Magnetic-Electrical Machines, without which those machines were comparatively useless as implements of Electro-Dynamic investigations. The invention is also of great use in bringing the whole of the excited force into play, either in Electro-Chemical operations or in telegraphic communications; and it is probable that in both these capacities, the Magnetic-Electrical Machine, with this appendage, will eventually supersede the employment of Voltaic Batteries.

These researches are fully described in the twelfth Memoir.

Note.—On the first of October, 1836, I commenced a new scientific Journal, called "*The Annals of Electricity, Magnetism and Chemistry, and Guardian of Experimental*

* Historical Sketch, page 4, note.

Science," and continued it, under my own superintendence, through ten octavo volumes. This is the first periodical especially devoted to Electrical and Magnetic discoveries. It became the medium of much valuable information. Its success gave rise to a similar work, called "*Archives de L'Electricite*," by Professor M. De la Rive, of Geneva, the first number of which appeared in 1841. Another, also, published in London, under the title of "*The Electrical Magazine*," took up a similar position—all of them convenient and valuable in conveying scientific information to the public.

27th. *An Inquiry into the Attributes of the Galvanometer.*

(Originally published in the Annals of Electricity, &c. for October, 1836.)

The Galvanometer is an instrument which, since the discovery of Electro-Magnetism, has been extensively employed for ascertaining the existence of electric currents, and the directions in which they flow. It has been made of various fashions, but invariably upon Electro-Magnetic principles. The Galvanometer here alluded to is that most usually employed: it consists of a hollow coil of wire, and a delicately-suspended magnetic needle, placed within it. When an electric current flows through the coil-wire, the magnetic needle becomes deflected from its previous position, and the direction in which it moves indicates the direction of the current.

The indications of this instrument are, however, of two distinctly different kinds. One of these is the *extreme range* to which the needle is deflected by the first sudden momentary impulses it receives from an electric current at the instant of its birth; the other is the *stationary position* of the needle which it assumes whilst under the influence of a continuous uniform electric current; or it is the steady position which the needle takes when it has ceased to oscillate from the first impulse of the current. And, as both these indications had been indiscriminately resorted to for the admeasurement of the *relative forces* of electric currents, in investigations that assumed a high importance, it became an interesting question how far such indications could be depended on, and what were their relations to each other. Such were the objects of the inquiry. The results are embodied in the eighteenth Memoir.

28th. *Researches on the Electro-Chemical Action exercised by simple Metals on Fluids.*

(Originally published in the Annals of Electricity, &c. for October, 1836.)

The first part of these researches were published in the pamphlet mentioned in Number 12, but the latter part did not appear till 1836. They now form the sixth Memoir.

29th. *Researches on the production of Electric Shocks by means of a single pair of Voltaic Metals.*

(Originally published in the Annals of Electricity, &c. for October, 1836.)

The account of these researches, intended for publication, was written in a great hurry, as the journal in which it appeared was nearly all printed at the time, and had to be published the third day after the latter part of it was composed, viz. on the first of October; and the paper was written on the night of the 28th of September. Therefore, like many others which are descriptive of novel facts, whilst experimental sciences are in the progress of cultivation, this part of my researches embraces some theoretical views which were afterwards found to require correction. The novel facts that were developed, however, led to further investigation, and eventually to the invention of the Electro-Magnetic Coil-Machine—an instrument which, from the time it first became known, has been found of great value in medical operations. This division of researches forms Part II. of the thirteenth Memoir.

30th. *Description of an Electro-Magnetic Engine for giving motion to Machinery.*

(Originally published in the Annals of Electricity, &c. for October, 1836.)

The engine here alluded to is, I believe, the first attempt on record to employ Electro-Magnetism as a motive power, in the shape of an engine for actual work. Professor Henry, of the United States, had previously constructed an Electro-Magnetic apparatus for giving a reciprocating motion to a lever; but, with this exception, if an exception it can be called, no attempt whatever had been made to bring Electro-Magnetism into notice as a motive force, previously to the construction of this engine, which is fully described in Section VI. of this volume.

31st. *Two Brilliant Electrical Experiments, well calculated for the Lecture Table.*

(Originally published in the Annals of Electricity, &c. for January, 1837.)

These experiments make an interesting addition to the series usually employed for illustrations at the lecture table; and the novel apparatus by which they are exhibited will be found worthy of admission amongst the old stock. The description forms one of the miscellaneous articles in Section VI.

32nd. *Experimental Researches on the Laws which govern the production of Electric Shocks, &c. from a single Voltaic pair of Metals.*

(Originally published in the Annals of Electricity, &c. for April, 1837.)

Previously to the commencement of this series of researches, which is a continuation of series 29, I had an opportunity of perusing Dr. Faraday's paper on the same subject,

which had been read before the Royal Society, January 29th, 1835. I am not aware of the date of the *Philosophical Transactions* in which Dr. Faraday's paper was published, but it was obvious that I was considerably behind him in the pursuit of the same object, although I had met with several facts which had not occurred to that philosopher. To a certain extent, however, we had pursued the same rout of investigation, which to me was no little mortification, because of an *apparent* plagiary, which I most abominably detest, and the neglect that I had committed by not making myself acquainted with what had previously been done, for I was even behind the American philosophers, who had their first hints from Dr. Faraday's researches.* However, I published the following acknowledgement the first opportunity that presented itself, which was in the *Annals of Electricity*, *&c.* for January, 1837 :—

" Since the publication of the first number of these *Annals*, I have seen, for the first time, that part of the *Philosophical Transactions* which contains Dr. Faraday's *Ninth series of Experimental Researches*, *&c.* ; and find in that series several experiments described, by which electric shocks are produced from the action of a single Voltaic pair, and other particulars relative to the powers of coils in the conducting circuit, similar to those described in my paper. I regret very much that I was not acquainted with those experiments before my paper was published, for it must be at all times an unenviable position for any one to be placed in, when in search of new facts, not to be acquainted with what had been done before; and more particularly so if he should happen to place in his own list of discoveries any of those which had previously been made, and which are justly and rightfully the property of others. It appears, however, that I have been led to some experiments which had previously been made by Dr. Faraday, and which have been attended with the same results as those discovered by that gentleman. So far, I take much pleasure in conceding what I have done; but there are other experiments detailed in my paper which have no bearing on Dr. Faraday's inquiries, and the views which we have taken of the nature of the action will appear perfectly distinct from each other. With regard to the difference of our results when using iron in the coils, I suspect it may probably be owing *partly* to the different fashion of our coils, and *partly* to the difference in the powers of the batteries employed. Dr. Faraday employed long, narrow coils, whilst those which I employed were the short, thick ones belonging to a Magnetic Electrical machine, and the iron I employed was the revolving armature belonging to them.

" I have not yet had time to repeat the experiments, but mean to do so shortly, and publish the results in the next number of the *Annals* ; and, in order to give that degree of credit to Dr. Faraday to which his experiments entitle him, I take much pleasure, at this earliest opportunity, in placing them before the readers of the *Annals of Electricity*, *&c.* in his own words."

* See Part II. of the thirteenth Memoir.

After this explanation, I placed Dr. Faraday's *Ninth Series*, alluded to, in the next following article, in the same number of the *Annals of Electricity*, *&c.* after which followed the present 32nd series of my own researches, which now form the fourteenth Memoir.

33rd. *On the application of the Theory of Magnetic Electricity to the phenomena exhibited by the Electro-Magnetic Coil Machine ; to Secondary electric currents ; and also to currents of the third, fourth, &c. orders.*

(Originally published in the Annals of Electricity, &c. for April and May, 1837.)

The object of this communication was to show that the same principles as those which I had employed in my theoretical views respecting Magnetic Electricity (see eleventh Memoir) were as applicable to the phenomena developed by *secondary* electric currents, &c. as to the *primitive* current, whether the production of a Voltaic battery or of a Magnetic Electrical Machine ; and, as currents of the second, third, fourth, &c. orders, presented a complexity of action which, at that time, had received no explanation, I considered this a fair opportunity to ascertain the correctness or incorrectness of the theory by the most rigid tests, as will be found by consulting the sixteenth Memoir.

34th. *Observations on the phenomena of Electro-Magnetism and Electro-Chemistry, by one and the same electric current.*

(Originally published in the Annals of Electricity, &c. for July, 1837.)

These observations, with some others on similar subjects, appear in Section VI.

35th. *Researches on the Attributes of the Galvanometer.*

(Originally published in the Annals of Electricity, &c. for July, 1837.)

These researches form a part of the eighteenth Memoir.

36th. *Researches respecting the Influence of Electric Currents on Soft Iron, as regards the thickness of the metal requisite for the full display of Magnetic Action ; and how far thin pieces of Iron are available for practical purposes.*

(Read before the London Electrical Society, August 5th, 1837, and published in the Annals of Electricity, &c. for October, 1837.)

In this series of researches, several facts respecting the action of coiled conducting wires were developed, that had not been arrived at in the 32nd series—such as the length and thickness of the coil-wire, and also the *disposition* of that wire most suitable for giving a maximum of action. Several interesting facts were also developed

respecting the action of soft iron, when employed in the interior, in different quantities and in different states of division. In fact, it was this series of researches that first gave the Electro-Magnetic Coil Machine an existence, and brought that valuable instrument to its present state of perfection. The mode of opening and shutting the battery circuit has undergone various modifications; but the philosophy and the fashion of the instrument still remain as these researches left them, the particulars of which are contained in the fifteenth Memoir.

37th. *An Address delivered to the London Electrical Society, October 7th*, 1837.

(Originally published in the Society's Transactions.)

See Miscellaneous articles, Section VI.

38th. *Researches concerning the Fracture of Leyden Jars by Electrical Explosions; with a Contrivance for preventing such accidents.*

(Originally published in the Annals of Electricity, &c. for February, 1838.)

This forms one of the miscellaneous articles in Section VI.

39th. *On the Production of Secondary Electric Currents in a Metallic Spiral, independently of opening and shutting the Battery Circuit, or of giving motion either to the Primitive or Secondary Conducting wire.*

(Originally published in the Annals of Electricity, &c. for February, 1838.)

This is probably the most satisfactory mode of illustrating the operation of the principles of the theory of Magnetic Electricity in Coil Machines. It will, therefore, be described in the sixteenth Memoir.

40th. *Experimental and Theoretical Researches in Electricity.*

(Read before the London Electrical Society, December 5th, 1837, and February 3rd, 1838; and published in the Transactions of that Society.)

The London Electrical Society having but recently been formed at the time this Memoir was read, and there being several of its members but little acquainted with Electricity, it was thought advisable to give a general view of the prevailing theoretical opinions, in connection with experimental facts, in a series of Memoirs, in order to give a stimulus to similar pursuits; and this was the first of the intended series. It now forms the twenty-second Memoir, in Section III. of this volume.

41st. *On Three different methods of Opening and Shutting the Circuits of Voltaic Batteries, for the production of Shocks and Brilliant Sparks or Scintillations.*

(Originally published in the Annals of Electricity, &c. for July, 1838.)

The descriptions are placed in Section VI.

42nd. *On a series of novel Electro-Calorific phenomena, developed by a Voltaic Battery.*

(Originally published in the Transactions of the London Electrical Society, for 1838.)

The experiments which developed these interesting phenomena were carried on at the house of J. P. Gassiot, Esq. on Clapham Common, near London. The battery consisted of 160 pairs of copper and zinc, placed in porcelain jars—having the copper placed in a solution of sulphate of copper, and the zinc in a solution of common salt, with a brown paper diaphram between the liquids. The battery was the joint property of Mr. Gassiot and Mr. T. Mason, both of whom were present, and assisted in the experiments. The particulars appear in Section VI.

43rd. *Observations on an Aurora Borealis, seen in the vicinity of London, Sept. 16th,* 1838.

(Originally published in the Annals of Electricity, &c. for October, 1838.)

See Section IV. Aurora Borealis.

44th. *On the Transfer of Liquid Conductors from one part of a Voltaic Circuit to another, by the force of a single pair of metals.*

(Originally published in the Annals of Electricity, &c. for March, 1839.)

See Section VI.

45th. *Description of a peculiar Voltaic Battery.*

(Originally published in the Annals of Electricity, &c. for March, 1839.)

See Section VI.

46th. *Researches on the Identity or Non-Identity of Electricity and Magnetism.*

(Originally published in the Transactions of the London Electrical Society, for 1838.)

This is the second Memoir of the series intended to be brought before the London Electrical Society. It comprehends rigid comparisons of a great variety of phenomena on both sides of the question, with many original facts which were developed during the inquiry. It now forms the twenty-third Memoir.

47th. *Experimental Researches on the Direct Action of Caloric on Magnetic Poles.*

(Originally published in the Annals of Electricity, for August, 1839.)

An account of these researches was read before the London Electrical Society, December 4th, 1848; but, having withdrawn my name from the list of members shortly afterwards, it was not published in their Transactions. It now forms the twenty-fourth Memoir.

48th. *On Marine Lightning Conductors.*

(Originally published in the Annals of Electricity, &c. for October, 1839.)

This Memoir was drawn up for the "British Association for the Promotion of Science," at their meeting at Birmingham, in September, 1839, but being too late was not read. It takes into account some experiments made by Mr. (now Sir W.) Snow Harris, at Plymouth, in presence of the Navy Board; an examination of the observed effects of lightning on shipping; a detail of other experiments, with remarks on the probable effects which lightning would produce on the conductors now adopted by the British Navy; and proposes a new system of conductors for shipping. The whole are contained in the twenty-first Memoir.

49th. *Observations on an Aurora Borealis, seen in London, September 3rd,* 1839.

(Originally published in the Annals of Electricity, &c. for March, 1840.)

See the description in Section IV.

50th. *Description of a Cast-Iron Voltaic Battery, with Experiments.*

(Originally published in the Annals of Electricity, &c. for July, 1840.)

The description is contained in the seventh Memoir.

51st. *Researches on the relative Powers of various kinds of Voltaic Batteries.*

(Originally published in the Annals of Electricity, &c. for August, 1840.)

Previous to these researches no attempt had been made to ascertain the precise extent of the Electro-Chemical powers of any individual Voltaic arrangement, nor had any means been taken to compare, with accuracy, the powers of different batteries in this capacity; and but very little had been done in forming a correct estimate of the powers of batteries in the production of any other class of Voltaic Electrical phenomena. With respect to Electro-Chemical action, it had generally been considered to increase with the extent of the battery series; and, with respect to the terminal metals in the liquid operated on, either thin pieces of gold or platinum wire, or narrow strips of those metals, had almost invariably been employed. The production of the Calorific phenomena had been better understood, but the maximum of electro-magnetic action which any battery would produce had never been a subject of strict inquiry. These researches, however, were directed to all these classes of phenomena, with four different kinds of battery. They form the seventh Memoir.

52nd. *A Letter to Professor Silliman, of Yale College, United States.*

(Published in the American Journal of Science.)

See miscellaneous articles, Section VI.

53rd. *Observations on an Aurora Borealis, seen at Manchester, October 19th*, 1840.

(Originally published in the Annals of Electricity, &c. for November, 1840.)

See Section IV.

54th. *Observations on an Aurora Borealis, seen at Manchester, December 21st*, 1840.

(Originally published in the Annals of Electricity, &c. for January, 1841.)

See Section IV.

55th. *On the Physical Theory of Electro-Magnetism, with its application to Phenomena.*

(Originally published in the Annals of Electricity, &c. for April, 1841.)

The principles of this theory are fully developed in the seventeenth Memoir.

56th. *An account of Damage by Lightning on St. Michael's Church, Liverpool*

(Originally published in the Annals of Electricity, &c. for November, 1841.)

The particulars are given in Section V.

57th. *Observations on an Aurora Borealis, seen at Manchester, April 5th*, 1843.

(Originally published in the Annals of Electricity, &c. for May, 1843.)

Described in Section IV.

58th. *On the Direct Action of Caloric on Magnetic Poles, and on the Displacement of Magnetic Action in Soft Iron, by the influence of heat.*

(Read before the Manchester Literary and Philosophical Society, November 29th and December 27th, 1842, and February 21st, 1843, and printed in their Memoirs.)

The first part of these researches have shown that the poles of a steel magnet are susceptible of translation, in a *lateral* direction, by the operation of heat; and obey the same law, with reference to the heated point, as their translation in a *longitudinal* direction, which was developed in the 47th series. The latter part of these researches develop some curious facts respecting the total absence of magnetic action in soft iron when made red hot, and by what means a bar may be converted into several magnets at one and the same time.

These researches are described in the twenty-fifth Memoir.

59th. *An Experimental Investigation of the Magnetic Characters of Simple Metals, Metallic Alloys, and Salts.*

(Read before the Manchester Literary and Philosophical Society, April 7th, 1846, and printed in their Memoirs.)

The general results of this investigation showed that several metallic alloys became endued with magnetic properties, although the constituents seperately show no such action; and that iron and nickel, two metals which, whilst pure, are susceptible of the highest magnetic powers, became almost totally inert to magnetic action when combined with some other metals. See twenty-sixth Memoir.

60th. *Observations on the Aurora Borealis, seen at Kirby-Lonsdale, Westmoreland, September 29th,* 1847.

(Originally printed in the Memoirs of the Manchester Literary and Philosophical Society, for 1848.)

Description in Section IV.

61st. *Observations on an Aurora Borealis, seen at Kirby-Lonsdale, Westmoreland, October 24th,* 1847.

(Originally printed in the Memoirs of the Manchester Literary and Philosophical Society, for 1848.)

Description in Section IV.

62nd. *Observations on the Formation of Clouds in the locality of Kirby-Lonsdale, Westmoreland.*

(Originally printed in the Memoirs of the Manchester Literary and Philosophical Society, for 1848.)

See miscellaneous articles, Section VI.

63rd. *An account of several displays of the Aurora Borealis, as seen at Prestwich, near Manchester, from the Autumn of* 1848 *till the Spring of* 1849.

See Section IV.

SECTION III.

SCIENTIFIC RESEARCHES, EXPERIMENTAL AND THEORETICAL. BY WILLIAM STURGEON. METHODICALLY ARRANGED, ACCORDING TO THE RELATIONS OF THE SUBJECTS, IN A SERIES OF TWENTY-SIX MEMOIRS.

PRELIMINARY DISSERTATION.

With the exception of the few experiments by Sir Humphry Davy and M. Arago, for magnetizing small sewing needles by discharges from Leyden jars,* the electric currents that were employed in the early part of the cultivation of Electro-Magnetism were the productions of Voltaic batteries. In the year 1822, however, Dr. Seebeck, of Berlin, discovered a new mode of producing electric currents, independently of any liquid being employed in the process, which, at first, flattered the hopes of philosophers of being enabled to supersede the tedious and expensive employment of Voltaic batteries in future inquiries of this kind. The plan pursued by the Prussian philosopher was simply that of producing an unequal degree of temperature in different parts of certain associations of two or more dissimilar metals, or other conductors of Electricity.†

The first apparatus of this class that appeared in London, consisted of a rectangular metallic frame, similar to the frame of a small picture, and a magnetic needle situated within it, as represented by Fig. 1, Plate I. The two consecutive unshaded sides were of bismuth, and the other two, which are shaded, of antimony, and the frame completed by soldering these two parts together at the angles, *e* and *f*.

* See the Historical Sketch of Electro-Magnetism, pages 13 and 14. † Historical Sketch, page 30.

When the plane of this rectangle is adjusted to the plane of the magnetic meridian, as ascertained by the magnetic needle, *n s*,* and the temperature of the angle, *e*, slightly elevated by the flame of a spirit lamp, the needle immediately turns out of its first position, and thus indicates the presence of an electric current, flowing throughout the metals composing the frame. If, instead of heating the angle, *e*, the lamp be applied to the opposite angle, *f*, the electric current thus produced will traverse the frame in the opposite direction to the former one, and give the needle a new position indicative of the direction of the latter current. The process of Seebeck has been termed *Thermo-Magnetic*, and by some philosophers *Thermo-Electric*, because of the employment of *heat*, and the production of electric currents. But electric currents may be produced in this apparatus independently of any additional heat being applied at the angles, *e* or *f;* for if either of those angles be cooled to below the natural temperature of the metals, by any process whatever, it amounts to the same thing as heating the angle opposite to it. So that if the angle, *f*, be cooled to below the general temperature of the system at the time of observation, an electric current would be produced as promptly, and in the same direction as if the angle, *e*, were heated; and by cooling *e*, instead of heating *f*, the opposite current would be produced.

The new field of inquiry thus laid open by the experiments of Seebeck soon engaged the attention of philosophers, both in this country and on the continent, each of whom contributed novel facts to those previously developed.† With respect to my own labours in this department of science, a minute description of the particulars attending them, and their several results, will necessarily be expected to appear in this volume; and as those labours had their commencement at an early period of the inquiry, and were subjects of the first papers that I ventured to make public through the medium of Scientific Journals, their chronological position entitles them to a precedence in the following series of Memoirs.

My first care was to facilitate the process in the original experiments of the Prussian philosopher, by giving a new form to the apparatus, as represented by Fig. 2, Plate I. in which the semicircular part is of bismuth, and the diameter, or straight bar, A A, is antimony; the two metals being soldered together at A and A, and held firmly in a grove at the top of a mahogany pillar, by means of a screw with a milled-head. By this form of the apparatus, the flame of a spirit lamp can easily be applied to either of the joinings, A or A', of the metals. I have also found it exceedingly convenient at the lecture table; for, by having a few dissimilar pairs of metals already mounted on their pillars, and properly arranged in their respective magnetic meridians, their *relative* electric action can be satisfactorily exhibited in the course of a few minutes, by a momentary application of the lamp to each pair.

* The magnetic meridian is a vertical plane passing through the poles of the needle: hence the needle, when unrestrained, always tends to rest in the magnetic meridian.

† See Historical Sketch, pages 30 and 31.

EXPERIMENTAL AND THEORETICAL RESEARCHES IN ELECTRO AND THERMO-MAGNETISM.

FIRST MEMOIR.

PART I.

Being desirous of ascertaining the relation that subsists between the Voltaic and Thermo-Electrical phenomena, as exhibited under the influence of a magnet, so as to form a comparison of the actions of the widely dissimilar apparatus for bringing the electrical powers into play; and likewise, if possible, to ascertain some general law to be observed in the production of phenomena by the employment of these dissimilar processes of excitation; the few following experiments were suggested as the most likely to lead to satisfactory results. As this topic of investigation appears to have escaped the attention of other experimenters,* a minute detail of those experiments, and a description of an instrument that I have been led to invent and construct, upon a very simple principle, for the purpose of carrying on similar investigations to any required extent, may possibly be found of some interest to scientific readers.

Experiment 1.—I charged, in the usual way, with diluted nitric acid, an Ampere's rotating cylinder, and placed it on a table. I now placed the north pole of a bar-magnet on the upper edge of the *outside* rim of the copper part of the apparatus. This done, I brought one of the wires belonging to the *zinc* cylinder directly opposite to the magnetic pole, and observed its deflection, which was to the *left hand* of a spectator, supposed to be situated in the centre of the cylinders, and looking towards the magnet. When the south pole of the magnet was presented to the wire, the deflection was towads the right. Fig. 3, Plate I. will afford a good idea of the position of the magnet with respect to the wires, z z', belonging to the *zinc* cylinder of the Voltaic apparatus, and the small darts will indicate the directions in which the electric currents flow in those wires; for it is a general law in Voltaic Electricity, that the electric current, produced with a combination of copper, zinc, and dilute nitric acid, flows through the conecting wire *from* the copper *to* the zinc, and through the acid liquor from the latter to the former metal. The horizontal arrow will indicate the

* I was not aware, at the time this paper was written, that Professor Cumming, of Cambridge, had been engaged in similar inquiries, consequently I had no means of knowing the results which that philosopher arrived at; and it was not till several months after its publication in the *Philosophical Magazine*, that I became acquainted with the Professor's researches, in which I found that in some of my own inquiries I had been anticipated. Professor Cumming's interesting papers on this subject appear in the second volume of the *Transactions of the Cambridge Philosophical Society*. (See Historical Sketch, page 30.)

direction in which the wire z' moved when the *north pole* of the magnet was presented to it, as in the former part of the experiment; and the opposite direction, of course, was that in which it moved when the *south pole* was presented to it, as in the latter part of the experiment.

Experiment 2.—I now suspended, by an untwisted silken fibre, a semicircular copper wire, with a zinc diameter (as represented by Fig. 4, Plate I.) in which the semicircle is the copper wire, and z z' a thin slip of zinc, soldered to the former at the extremities z and z'. The north pole of a magnet, M, was now placed close to one arm of the semicircular arc, as shown in the figure, and the flame of a spirit lamp applied at z', the joining of the metals nearest to the magnet. A deflection of the suspended system of metallic wires immediately took place, and the branch to which the lamp was applied moved towards the *left* hand of an observer, supposed to be situated in the centre of motion, and looking towards the magnet.

Experiment 3.—The semicircle was adjusted to the magnet, as in the previous experiment, but the lamp was now applied at z, or that joining of the metals most remote from the magnet. In this case the branch to which the magnetic pole was presented was deflected towards the *right.*

Experiments 4 *and* 5.—In these experiments the lamp was applied as in the two preceding ones, but the poles of the magnet were reversed—the *south pole* being that presented to the suspended apparatus. The motions which the wire exhibited in these cases were the reverse of those produced by the influence of the *north pole* of the magnet, and consequently indicative of electric currents *similar* to the former, by similar applications of the lamp.

I have made similar experiments with rectangles, and other shapes of the combined or associated metals, besides those of the semicircular form, but I have not found any difference in the results; so that the thermo-electric actions of metallic associations do not appear to be modified by peculiar fashions given to the apparatus.

If a semicircular or rectangular apparatus consisted of platinum and silver wires, (the former supplying the place of the copper, and the latter substituted the zinc in the preceding Thermo experiments,) the results with the two pieces of apparatus would be precisely alike: hence the platinum has the same electrical relation to the silver as the copper has to the zinc.

But this is not the only circumstance to be noticed in these experiments; we must compare the results that have been developed by the Voltaic and Thermetic processes of exciting the electric currents, and endeavour to trace the relations that subsist in the phenomena thus obtained. For this purpose we may compare Experiment 1 with Experiment 2, in both of which we find the conducting wires, to which the *north pole* of the magnet was presented, deflected in the same direction; and hence we are led to infer, in the first place, upon the well established laws of Electro-Magnetism, that

the electric currents were transmitted along those branches of the wires situated nearest to the magnet, in *one* and the *same* direction.

If, now, we trace the direction of the conducting wires from their respective points of excitation—one from the acid solution and the other from the point of heat—we find that, in the former case, the conducting wire ascends from the zinc, but, in the latter, it ascends from the copper. Therefore, since the current runs *downwards* in both conductors, its course through the point of heat, by the Thermo process, is *from* the copper to the zinc ; whereas, the direction of the current through the acid liquor, in the Voltaic apparatus, is *from* the zinc to the copper. Hence we learn that the two processes give different results, the currents they produce being propelled in opposite directions to each other ; and, as I have obtained similar results by the employment of a variety of other metallic combinations, I have been led to the conclusion that the above law is general : that is, whatever be the character of the metals employed in pairs, the Voltaic and Thermic processes would produce electric currents in opposite directions to each other, under the consideration that the acid liquor in the former case, and the point of heat in the latter, are respectively the sources of electric excitement.*

Description of an Electro-Dynamoscope, an Instrument for the purpose of exhibiting and compariug the Directions of Electric Currents produced by the Voltaic and Thermic-processes of Excitement.

Fig. 5, Plate I. represents an elevation of the Instrument, in which *c c* is a cylindrical glass tube, furnished with a brass cap, *f f*, at its upper end, and a brass rim, *c a'*, at its lower end. The rim is furnished with a brass arm, *c a'*, attached to it ; and the cap, *f f*, is furnished with a stout wire, *w m*, with a milled-head at *m*, and supported by two brass studs, in which are perforations for its reception. One end of an untwisted silken fibre is fixed to the middle of the wire, and the other end is supplied with a fine silver-wire hook, which descends through a small hole in the centre of the brass cap into the axis of the glass cylinder. The pillar and its foot, with its circular table, *t t*, and its bent arms, *a a'*, are all of brass, and constitute the support for the glass cylinder and its appendages—the one being firmly attached to the other by means of a thumb-screw at *a'*. The circular table, *t t*, is furnished with a flat moveable rim, which is graduated, in both directions, from 0° to 90°, for the purpose of measuring the quantity of deflection. A space of about half an inch intervenes between the circular table, *t t*, and the lower end of the glass cylinder, which affords a free motion to the thermo-electric wires.

* Professor Cumming seems to have found a few exceptions to this law in certain combinations of metallic bodies.

Figs. 6 and 7 show the fashions of the *Thermic* and the *Voltaic* combinations respectively. In the former, P P is a platinum or other wire, bent as in the figure, having its extremities twisted to a straight wire, *s s*, of silver, or some other metal. When this combination of metallic wires is used for experiment, the glass cylinder of Fig. 5 is taken off its support, and the wires suspended within it by the silken fibre and its silver hook, and the whole replaced, as represented by Fig. 5. When the suspended wires have come to rest, (which soon takes place, for the glass cylinder prevents the interference of undulations of the external air) the graduated circle is to be adjusted, the mounted magnet, N S, placed as in the figure, and the flame of the lamp applied at *l*.

In Fig. 7, *c* and *z* represent two morsels of copper and zinc wires respectively, connected together by the conducting wire, *w w*, and kept parallel to each other by means of a piece of cork. When used for experiment, this Voltaic combination is to be suspended by the silken fibre, within the glass cylinder, Fig. 5. The glass vessel, *v*, holding acid liquor, is placed on the centre of the table, *t t*: the Voltaic metals are then let down to any required depth into the acid liquor, and the magnet applied as before.

This method of detecting feeble electric currents is so efficacious, that a piece of fine silver wire, such as forms a part of what is called gold-lace, with a piece of zinc of the same dimensions, immersed in diluted acid not more than one-tenth of an inch of their length, will cause the connecting wire, on the approach of the magnet, to be deflected 40° or 50° from its first position, or zero of the graduated plate.

I have a geat variety of metallic combinations ready formed for experiment. They are placed between the leaves of a small book, made for the purpose of keeping them from injury, with the names of the metals on the leaves where they are respectively deposited. The whole packs up in a neat mahogany box.

This instrument differs from all the Galvanometers that I have yet seen described. I employ a powerful magnet for detecting and ascertaining the direction of feeble electric currents; whereas, in the Galvanometer, a feeble magnetic needle is used for that purpose. Many other differences are easily discoverable in the two kinds of apparatus.

Woolwich, *February*, 1824.

EXPERIMENTAL AND THEORETICAL RESEARCHES IN ELECTRO AND THERMO-MAGNETISM.

FIRST MEMOIR.

PART II.

Although the experiments I have detailed in the former part of this Memoir, have sufficiently satisfied myself with respect to the directive exertion of the electro-magnetic forces, in the two differently excited combinations of metallic wires, yet it is not impossible that some doubts may be entertained respecting the competency of those experiments for authorizing the conclusions at which I have arrived, owing, it may possibly be supposed, to the difference in the structure of the apparatus employed in the investigation. In order, therefore, to obviate all difference of opinion respecting the inferences I have drawn, and the views that I have taken, the following experiments have been instituted:—

Suspend, by a silken fibre, the semicircular copper arc, *c*, with its zinc diameter, z z, as represented by Fig. 4, Plate I.; but, instead of soldering the metals together, merely twist their extremities one over the other, as shown by the figure.

Wrap one of these joinings of the metals loosely with a piece of tow, or unspun cotton. Dip this part of the combination into diluted nitric acid, and counterpoise by attaching something to the other end. Present the *north pole* of a bar magnet to the arm that *ascends* from the acid solution, and that arm will be deflected towards the *right.*

Take off the cotton or tow, wash and dry the wires, and suspend them as before. Apply now the flame of the lamp, instead of the acid solution, and the magnet as in the former experiment: that arm of the apparatus will now be deflected to the *left.*

This method of experimenting admits of no ambiguity in its results, on which account it may possibly be better adapted for comparing the phenomena than by the experiments previously described, which was the principal object for resorting to it. But the *Electro-Dynamoscope*, previously noticed, and represented by Fig. 5, Plate I. admits of the most eligible mode of experimenting when the electric forces are very feeble.

The results obtained from the foregoing series of experiments could hardly fail to suggest the idea of increasing the electric forces in a combination of wires, by applying an acid solution at one of the joinings, and heat at the other; which, by a few experiments with the apparatus already described, was found to be correct.

The copper wire was bent into the shape of P P, Fig. 6, Plate I. and twisted a good length round the ends of the zinc, *s s*, of that figure, in order that a considerable extent of metallic surface might be exposed to the acid liquor. After moistening the tow, which was wrapped round one of the joinings, with acid liquor, the machine was suspended in the apparatus represented by Fig. 5. On presenting the north pole of the magnet to the arm *ascending* from the moistened tow, the deflection was to the right about 80°. From that point the wire returned to about 20°, thence propelled again to nearly the same angle as at first; and thus it vibrated several times through an arc between 15° and 60° from the magnet. At this stage of the experiment, I changed the poles of the magnet, presenting its south pole to the same arm of the suspended apparatus as had been previously exposed to the influence of the north pole. The deflection of about 70° now took place in the opposite direction to the former; and, as in the former part of the experiment, the first deflection was met by the force of torsion in the silken fibre; by means of which, and the force of the magnetic pole, a series of vibrations succeeded, which gradually lessened, and the arcs of description kept approximating closer to the zero point.

When the electro-magnetic force in the wires had become so feeble that the greatest deflection did not exceed 30°, I applied the lamp to the other extremity of the metallic combination, which almost immediately increased the deflection to above 100°; and, by keeping that extremity *warm* (for the zinc will not withstand a strong heat) I could keep the suspended wires vibrating at about 90° from the pole of the magnet, or zero point. When the wires were deflected to above 90°, as was sometimes the case, the electro-magnetic forces of the other branch, in consequence of its approach towards the magnetic pole, now conspiring with the force of torsion, suddenly reduced the deflection to about 40° or 50°; but, by keeping the joining of the wires moderately warm, the plane of the suspended system could be kept nearly at right angles to its neutral or normal position—the deflection never amounting to less than 80°.

I now again changed the pole of the magnet, and took away the lamp. The Voltaic electrical action had now become so feeble as to be but just discernable; but, by replacing the lamp, the wires soon acquired a deflection of between 60° and 70°, which could be kept up to nearly the lower point by a continuation of the heat.

I have repeated this experiment, with the same results, in about six-and-a-half minutes each time. The copper wire was about one-sixtieth of an inch diameter, and the zinc about twice that thickness.

When the flame of the spirit lamp is applied before the Voltaic action gets too weak, the results of this experiment in miniature are beautifully impressive; and so happily conclusive respecting the directions of the currents through the system of wires, and of the capability of uniting the electric forces produced by the two dissimilar modes of excitement; also in such a manner that they may concur in direc-

tion and become productive of confluent electric currents, that the mind at once becomes reconciled with the facts, and reposes with confidence on the inferences they so obviously lead to, independently of further demonstration. It must be confessed, however, that these experiments have been made upon a completely miniature scale; and, as I have no means at present to carry on the investigation to a greater extent, or with large metallic combinations, I am not prepared to say what would occur under those circumstances—although, from facts already known, we have no right to suppose that the *character* of the action would vary by a difference in the size of the metals employed.

The electro-magnetic forces developed by the Thermo-process, appearing so perfectly analagous to those due to Voltaic excitement, with the exception of direction in which they are exerted, that there appeared a probability of accomplishing rotatory movements by the former, similar to those that have been produced by means of the latter mode of excitement: and, notwithstanding the unsuccessful attempts that have been made by philosophers eminent in this branch of physics, I have fortunately succeeded in developing the analogy sought for in this peculiar and interesting class of phenomena—the rotation being accomplished by the influence of a central magnet.

Woolwich, *April*, 1824.

EXPERIMENTAL AND THEORETICAL RESEARCHES IN ELECTRO AND THERMO-MAGNETISM.

FIRST MEMOIR.

PART III.

Having stated at the close of the Second Part of this Memoir, that I had succeeded in producing a Thermo-Magnetic rotation around the pole of a *central* magnet, I will now proceed to describe the apparatus by means of which that motion was accomplished.

N S, in Fig. 8, Plate I. represents a portion of the vertical magnet; P C P, a piece of platinum wire, bent into the form of a semicircle, or other convenient curve; P S, P S, are two pieces of silver wire, twisted to the former at the extremities P and P. The other ends of the silver wires are bent downwards at S and S, and made quite sharp and smooth at the points. These points descend into the metallic cell, F E, which contains pure quicksilver, with which the points communicate. A descending point, C, soldered to the platinum wire, forms a pivot on which the moveable part of

the apparatus turns. A small cavity, well polished at the bottom, is made in the end of the magnet, for the purpose of containing a small globule of quicksilver and the rotating point to work in.

The point, C, being amalgamated and placed in the globule of quicksilver, forms a communication between the platinum wire and the magnet; and the points, S S, of the silver wire, communicating with the quicksilver in the cell, F E, and the cell itself with that part of the magnet which passes through it, the metallic circuit becomes thus complete—first, through the platinum wire from P to C, thence through the pivot to the top of the magnet, and along the upper part of the magnet down to the quicksilver in the cell, F E; and, lastly, along the silver wire, from the point S, to the extremity at P, where it is united with the platinum wire. The arrangement of the metals is precisely the same on both sides of the pivot, C, so that the integrity of conduction is complete and uniform in both branches of the system.

Now, since both branches of this Thermo-Magnetic apparatus are identical in every respect, they are strictly assimilated to the arms of the rotating cylinders of Ampere, with respect to *position* as conductors from the respective sources of excitement; for, when the unions of P and P of the platinum and silver wires become heated by a spirit lamp, the electric currents, thus excited, flow along both arms of the platinum wire in one and the same direction—downwards; therefore, the tendency to rotation is equal and uniform on both sides of the magnet, and the revolving motion may be continued by continuing the heat at the points P and P.

The moveable part of the apparatus, which consists of the platinum and silver wires only, will rotate on the magnetic pole with a facility proportioned to the delicacy of suspension, the difference of temperature of the parts P and C of each arm, and the power of the magnet. But I must here warn the reader that dexterity in the experimenter is not the least of the requisites to ensure success in the experiment; for, had I not been perfectly satisfied that the apparatus was constructed upon principle, I might probably not have persevered sufficiently to attain my object. However, by a trifling modification of the apparatus, the experiment is considerably facilitated and the rotation rendered more permanent and beautiful.

The modification alluded to consists of a circle of small spirit lamps, placed on a round annular stage, in such a manner that the centres of their flames may coincide with the periphery of the circle described by the points P and P of the apparatus, Fig. 8, so that these points or joinings of the dissimilar metals may be kept at nearly the same uniform temperature in every part of their revolution; and the shoulder of the arms, or that part to which the pivot, C, is soldered, is kept as cool as possible by means of æther, or other cooling liquid.

Another improvement of the apparatus is accomplished by having a conducting wire from the pivot, C, to the metallic cell, F E, in the same manner as the conducting

wire attached to the *copper* part of Ampere's rotating cylinders. Through the upper part of this conducting wire passes a small screw, with a milled-head, formed into a cup on its upper side. The pivot, C, runs in this cup, at the bottom of which is placed a small globule of quicksilver, for the greater security of metallic contact. The cup is then filled up with æther, which may be replenished during the period of an experiment in proportion to the evaporation.

The lower end of this small screw rests in the hole in the top of the magnet; and, by turning its milled-head to the right or the left, the points, S S, of the silver wires may be elevated or depressed at pleasure, and consequently their depth of immersion in the quicksilver in the cell, F E, may be regulated to the greatest degree of nicety—the attainment of which was the greatest embarrassment I had to encounter with the original apparatus. However, by perseverance and determination, every obstacle was overcome, and I had the satisfaction of witnessing, in this experiment, the first Thermo-Magnetic rotation ever produced by the influence of a central magnet. By this experiment, a new parallel with the well-known phenomena of Electro-Magnetism has become established, and this infant science advanced another stage in the progress of its cultivation.

P.S.—*April* 13*th*, 1824.—I have, since the above was written, succeeded in forming a Galvanized sphere of wires, which rotates on its axis by the influence of *both* poles of a central magnet. The experiment was suggested on reading Dr. Halley's theory of the earth; and, though it may not be considered as tending to prove the correctness of that philosopher's views of terrestrial Magnetism, it may, perhaps, appear somewhat favourable to them.

[*A description of this apparatus will appear in the third Memoir.*]

SUPPLEMENT TO THE FIRST MEMOIR.

Shortly after the Third Part of this Memoir was published in the *Philosophical Magazine*, I gave a new fashion to the apparatus there described, or rather made a novel and perfectly distinct instrument for the display of Thermo-Magnetic rotations; thus conferring a new interest on these phenomena, and a more favourable reception at the lecture table.

The apparatus is represented by Fig. 9, Plate 1. The horse-shoe magnet is held in a vertical position by means of a brass stand, as shown in the figure; and on each pole is attached a rotating piece, P P, S S, consisting of platinum and silver wires; an

annular brass vessel, *c*, for holding quicksilver; and an annular brass box, or reservoir, *r r*, for holding spirit of wine, and supplying eight small burners which rise from its upper flat *surface*.

The reservoir, *r r*, is fixed to the magnet by means of a thumb-screw, and the annular cell, *c*, rests upon it. The silver and platinum wires are twisted together, in the manner already described—[brazing the wires together is preferable]—and the circuit is completed by means of the pivot, the screw-head in which it revolves, the wire in which the screw works, and which reaches down to the mercury in the cell, *c*, and through the mercury itself to the point of the silver wire.

Previously to lighting the lamps, the cups in which the pivots work, should be filled with æther, which, by evaporation, tends to keep the upper parts of the apparatus at a much lower temperature than the lower parts, where the silver and platinum are united. On lighting the lamps and bringing the junctions of the wires into the flame, the two systems of wires immediately commence their revolving movements; and, as the electric currents flow in similar directions, and under the influence of dissimilar magnetic poles, the revolutions of the two Thermo-systems are the reverse of each other, in accordance with the laws of Electro-Magnetism.

When this apparatus is placed alongside of that which is represented by Fig. 6, Plate VI. and both in full play, they present a most interesting and captivating spectacle, even to ordinary observers; and well calculated to command the admiration of the profoundest philosopher.

Rotation of a System of Rectangular Conducting Wires, carrying Thermo-Electric Currents.

The rotations hitherto described have all been performed by those metallic wires in which the electric currents were generated; and in no case have such motions been shown by Thermo-Electric currents, whilst the batteries or Thermo-metals continue at rest—a fact still wanting to perfect the analogy of the phenomena displayed by this process, and those already developed by the employment of the Voltaic battery. The experiment now about to be described will assist in filling up that gap.

In Fig. 9 of Plate III, *b* and *a* are intended to represent two parallel bars of metal, joined together by solder at the end *l*, but insulated from each other by an intervening strip of card-board the remaining part of their length, as indicated by the broad blank line. This Thermo-combination consists of bismuth and antimony; the former, being the more fusible of the two, is placed uppermost, to keep it out of the flame of the lamp, which is placed under the antimony at the point *l*. To the other

ends of the metals are soldered copper wires, one to each, having small cups at their extremities for the purpose of holding mercury; the ends of the wires pass through the bottoms of the cups, and thus come into contact with the mercury. The whole is kept steady by means of a wooden stand, as represented in the figure.

In the other part of the figure, the rectangular piece, *b c d e*, consists of brass wire, which terminates in two points—downwards, so as to just touch two narrow oblong portions of mercury, held in gutters on the top of the wooden stand; but, as the rectangle can rotate on a pivot, these points touch the mercury only whilst in the position represented by the figure—in all other positions they are quite free from touching any thing. Parallel to, and directly over the mercurial gutters, are placed two bar magnets, sustained firmly in that position by means of a wooden frame, which is fixed by a thin leg to the top of the pillar; and, on the top of this frame, is a glass or agate pivot-hole, to receive the pivot of the rectangle. The ends of two short wires pass through the side of the upper part of the pillar and communicate with the mercurial gutters, and their exterior ends carry small cups containing mercury, with which they also communicate. When these cups are united with those of the antimony and bismuth, by connecting wires, in the manner represented by the figure, the circuit is completed. If, now, the spirit lamp be applied at *l*, a thermo-electric current will be produced, which will pass through the moveable rectangle, *b c d e*, which, on the principles of Electro-Magnetism, will be deflected from the poles of the magnets, and with a sufficient force to carry it through half a revolution; so that the depending points will again come into contact with the two portions of mercury, and close the circuit as before, with the rectangle in the reverse position. As soon as the circle is again completed, the rectangle receives a new impulse, and is again carried through half a revolution, at the end of which the propelling force is repeated; and thus, by a succession of impulses, a continuous rotation is accomplished.

When two of these rectangles are placed at right angles to each other, over the centre of motion, the impulses are given at every 90° of the described circle, and the rotation is more uniform than by one rectangle only.

ON THE THERMO-MAGNETISM OF HOMOGENEOUS BODIES WITH ILLUSTRATIVE EXPERIMENTS.

SECOND MEMOIR.

1. Eight or nine years have now elapsed since Dr. Seebeck, a Prussian philosopher, unfolded a most important secret of nature, by the discovery of magnetic powers in various metallic combinations, by merely submitting their points of union to different degrees of temperature—a discovery of equal, if not superior interest, to that of Electro-Magnetism by Œrsted, the illustrious Dane. Each of those discoveries marks a distinct and important epoch in the History of Experimental Science, and each philosopher now enjoys that degree of fame to which he is so justly entitled.

2. Philosophers in every civilized country have repeated the experiments of those celebrated men ; admired the beauty and interest of the phenomena they present, and vied with each other in adding new facts to those already known. Heat, Magnetism, and Electricity, are now blended in our experiments; and new sciences have been reared upon the phenomena they have jointly presented to our notice.

3. The discovery of Œrsted, and the train of curious and interesting phenomena to which it has directed our view, rest principally upon the action of metallic combinations, and a mode of excitation which, to philosophers, has long been known; whilst the discovery of Seebeck, on the contrary, not only depends upon new arrangements, but also upon the novel mode by which he calls forth the electric powers, and interrogates the magnetic phenomena they display.

4. In this mode of research, however, as well as in the former, combinations of two or more distinct metallic bodies were still employed; and it appears that, by whatever ingenious contrivances other philosophers have pursued the inquiry, combinations of two or more bodies have generally been considered necessary for the display of thermo-magnetic phenomena. I am not aware, however, that any experiments are yet before the public illustrative of the thermo-magnetic action in one solitary piece of metal, or other homogeneous body.

5. In my " *Recent Experimental Researches in Galvanism, Electro-Magnetism, &c.*" a small work lately published, I have stated that simple metallic bodies not only display different electric powers as regards each other, but also that the various *parts* of *each separate metal* are *relatively* in different electric states at one and the same time, although in close connection with each other by the best known conducting materials; that is, by the metal itself. And I have shown by several experiments on homogeneous

metals, that those magnetic powers, which are regarded as inseparable from the electric, can readily be brought into play either by the Galvanic or by the Thermo-process; a circumstance which to me appeared highly confirmatory of the hypothesis.

6. Since writing that work, I have been induced to prosecute those inquiries to a still greater extent; and the experiments and observations which are now about to be described, appear to me to permit no further question as to the existence of thermo-magnetic powers in most, if not in all, of the homogeneous metals—individually, and independently of any connection with each other. And the phenomena they display are, in many cases, as decidedly obedient to certain unerring laws, as in any thermo-magnetic arrangement whatever.

7. My first experiments, for the detection of Magnetism by heat in single pieces of metal, were not very successful; although the pieces were alloys, and consequently not homogeneous metals. I found, however, after some trials, that by hardening one end of a piece of steel, and keeping the other end quite soft, the thermo-magnetic energies were always called into play when any part near to the centre of the bar was heated, and the extremities left at the ordinary temperature of the atmosphere.

8. Brass also, by the same treatment, displays thermo-magnetic properties, which are easily detected by the Galvanometer. It is a remarkable fact, however, that the electric current in these metals proceeds in opposite directions, as regards the hard and soft ends. In cast steel the current in the bar is from the *hard* to the *soft* part; but in brass it flows in the contrary direction. Fig. 1, Plate II. is the shape of the steel or brass bar, which I find very convenient in these experiments: the extremities, which are a little bent, dip into the cups of the Galvanometer. There is no occasion, however, to employ a Galvanometer with brass. The bar, when heated at the bend, may have its extremities brought into close contact, and one side held over and parallel to the compass needle; and the nature of the deflection will indicate the direction of the electric current. When the bar is of steel, the direction of the electric current will be indicated by the arrow: when brass is employed, the current flows in the opposite direction.

9. I have magnetized the steel in all the various ways that I could think of; but I have not found that its being a magnet has any perceptible influence, either on the *direction* or the *power* of the electric current excited within itself by heat.

First Class of Experiments with single pieces of Bismuth.

10. Bismuth is one of the metals wherein the magnetic powers are finely developed by heat: the energies are promptly displayed, even by small specimens cast into certain forms; and, as their character can be examined without the aid of a multiplying

Galvanometer, the phenomena are very easily exhibited. Experiments on bismuth are, therefore, well calculated to impress immediate conviction on the mind, as to the distinguished and interesting character of the Thermo-Magnetism of homogeneous metals.

11. The first piece of bismuth which I employed in these researches was cast into the shape of a rectangular frame, not very unlike the rim of an old fashioned knee-buckle. Each side of this frame was a rectangular prism, the faces of which were each 0·3 of an inch broad. The sides, however, were not very smooth, but no file was employed to level the inequalities. The length of the frame outside measured 3·2 inches, in breadth 1·2 inch. The experiments were made by heating, at different times, various parts of the frame in the apex of the flame of a spirit lamp; and, when any selected point was thus heated for a few moments, one of the longest sides was immediately held over, and parallel to, a delicately suspended compass needle—on which, the Magnetism of the earth was pretty accurately neutralized by means of a distant bar magnet—and the deflections of the needle were taken as indications of the direction in which the electric currents flowed round the metallic frame.

12. The needle which I have employed is four inches long, furnished with an agate cap, and suspended on a fine steel point; it is also inclosed in a box, with a glass cover. Care was taken to neutralize the metallic frame between every two experiments, by plunging it into cold water.

13. Fig. 2 will represent this frame of bismuth. It was heated successively at the points, *a b c d*, close to the angles, but those angles were kept out of the flame of the spirit lamp. These points were selected for the points of heat, from a notion which I had previously entertained, that heat might possibly be obstructed by turning sharp angles, and thereby influence the direction of the electric currents to which it gave birth.

When the point of heat was at	*a*, the current flowed from	*b* to *a*
	b, ...	*b* to *a*
	c, ...	*b* to *a*
	d, ...	*a* to *b*

To prevent confusion, the side, *a b*, only, is chosen to show the directions of the electric currents. It is to be understood, however, that those currents were continuous round the rectangular frame.

14. When the experiments were repeated, the direction of the current changed when the point of heat was at *b*; at all the other points the current proceeded as at first. By varying the situation of the point of heat several times near to *b*, it was found that, when applied anywhere within half-an-inch of the angle, the current was from *b* to *a*, as at first; but, when the point of heat was one inch distant from the

angle, *b*, the current invariably proceeded from *a* to *b*. From these results, it was evident that between half-an-inch and one inch from the angle there was a point which, if heated, no electric current would be excited. By various trials this *neutral point* was found to be situated at a little more than half-an-inch from the angle, *b*. So that, in general, if all the first half-inch were heated, the current would proceed from *b* to *a*; but, if the more distant half-inch from the angle was heated, the current flowed in the opposite direction. Again, as this last current was also in the opposite direction to that excited by heating the point, *a*, there would evidently be another *neutral point* still nearer to *a*. This point was determined at nearly half way between the angles, *a* and *b*, but a little nearer to the former than to the latter.

15. In this way the situation of the point of heat was varied in the side, *c d*. One *neutral point* only on this side was detected, which was nearly half way between the angles, *c* and *d*. If any point in, or the whole of the half nearest to *c*, were heated, the current proceeded from *d* to *c*; but, if heat was applied to the other half nearest to *d*, or to any particular point in that half, the current flowed from *c* to *d*.

16. By consulting the direction of the arrows in Fig. 2, the reader will easily ascertain the direction of the electric currents when any particular part of the apparatus is heated; for, by heating any point within the range of any individual arrow, that arrow will point out the direction of the electric current. Or, if the whole length of that arrow be heated at the same time, the current still flows in the same direction, and is continued in every part of the frame. The same explanation will also apply to all the other figures, unless otherwise expressed.

Second Class of Experiments.

17. These experiments were also made with a rectangular frame of bismuth, exactly of the same length as the former, and about 1·75 inch broad. More care was taken in the casting, and the frame was a better figure.

18. The direction of the arrows in Fig. 3 will point out the course of the electric current when any point opposite to them is heated. It will be observed, by consulting that figure, that every angle is a *neutral point*; and that the long sides have each one *neutral point*, and when this is heated no current is excited.

19. There is a circumstance connected with these experiments which is well worthy of remark. On the side, *a b*, of the rectangle, and near to *b*, there was a protuberance on the inner face: when the point of heat was between this protuberance and the angle, *b*, no current was excited; but on the other side of the protuberance being the point of heat, a powerful electric current was put into motion; so that, strictly speaking, the arrow ought not to reach to the angle, *b*. The protuberance was afterwards

filed down, and that part levelled with the rest of the side: no difference was produced in the thermo-magnetic character of that part of the frame by the change thus made. Hence it would appear, that the internal structure of the metal alone operates in giving direction to electric currents excited by heat. This opinion will appear much better supported by experiments and observations, which will be spoken of in the sequel.

20. I must notice in this place, that the thermo-magnetic energies in this rectangle vary considerably by heating different points. When the point of heat is in the side, *a b*, the current which sets in from *b* to *a* is much more powerful than the opposite current from *a* to *b;* so that, by heating any part in the half nearest to *a*, a stronger current is excited than by heating a point similarly situated with regard to *b*.

21. When either of those halves is uniformly heated, the needle is more deflected than if the heat were confined to a single point. When the whole side, *a b*, is uniformly heated, in consequence of the superior energies attributable to the half neares to *a*, the current is directed from *b* to *a;* but the energies of this resulting force are necessarily very feeble.

Third Class of Experiments.

22. Three rectangular frames of bismuth, which I shall distinguish by the letters A, B, C, were cast in the same mould, and from the same mass of metal. The longest side of each frame measured 4·5 inches, and the shorter sides two inches. The weight of the whole was twenty-one ounces; so that the average weight of each was seven ounces. They were cast under different circumstances, as regards the temperature of the mould, agitation, position, &c.

23. The rectangle A was cast whilst the mould was quite cold; its plane horizontal, and its longest sides parallel to the magnetic meridian. The metal was all poured into the mould (which was simply a groove in one of the flat sides of a Bath brick) at one particular point, which was in one of the longest sides of the rectangle, and about 1·5 inch from one of the angles. The metal consequently flowed from this point to every other part of the mould. Two powerful bar magnets were applied to the two longest sides, whilst the bismuth remained in a fluid state; they were drawn from the centre of each long side of the rectangle, in precisely the same manner as is practised in magnetizing a needle by the double touch. The process was continued till some time after the metal was set: the mould was kept perfectly at rest all the time.

24. The rectangle B was cast while the mould was quite warm, from the heat communicated to it by the last casting. The magnetizing process was carried on as before, but the metal was agitated as much as possible all the time. The position of the mould was the same as whilst casting the rectangle A. The magnets in both cases

were so applied, that if the Magnetism of the earth had any influence in arranging the particles of the fluid metal,* those artificial magnets would have tended to promote that arrangement.

25. The rectangle c was cast whilst the mould was quite hot, and with its longest sides at right angles to the magnetic meridian. No magnet was applied to the bismuth, but continued agitation was kept up till long after the metal was set.

26. *Experiments with the Rectangle A.*—The arrows in Fig. 4 will, in general, indicate the direction of the electric currents in this piece of bismuth. There are, however, circumstances connected with the experiments which require some further explanation. I shall endeavour to point out these particulars with some degree of minuteness, commencing with those which were observed when the point of heat was on the side, *a b*, and proceed successively with the other sides, according to the regular order of the letters.

	SIDE *a b*.	CURRENT.	
Point of heat	Close to *a*	none	from *a* to *b*
	One inch from *a*	very feeble	
	Centre of *a b*	more powerful	
	One inch from *b*	very powerful	
	Close to *b*	very powerful	from *b* to *a*

Neutral point 0·5 inch from *b*.

	SIDE *b c*.		
Point of heat	Close to *b*	powerful	from *b* to *c*
	Centre of *b c*	powerful	
	Close to *c*	powerful	from *c* to *b*

Neutral point 0·25 inch from *c*.

	SIDE *c d*.		
Point of heat	Close to *c*	powerful	from *d* to *c*
	One inch from *c*	feeble	
	Centre of *c d*	powerful	from *c* to *d*
	One inch from *d*	more powerful	
	Close to *d*	powerful	from *d* to *c*

Neutral points 0·75 inch from *d*, and 1·5 inch from *c*. The latter was the gate or point at which the metal entered the mould.

Side *d a*.—To whatever point of this side of the rectangle heat was applied, the electric current flowed from *a* to *d*; but the energies were more powerful when the point of heat was close to *d* than when in any other part of the side *d a*.

* From some previous observations, I had formed an idea that this might possibly be the case.

27. It will be observed that in this rectangle there are only two of the angles, *a* and *b*, which are neutral points. The other angles, *c* and *d*, when heated, produce very powerful electric currents; indeed, much more so than when the point of heat is in any other part of the metallic frame. When those two angles are heated at the same time, their energies, being excited in the same direction, conspire to produce effect on the needle, which becomes much more deflected than when one angle only is heated. Precisely the same thing happens when any other two or more points are simultaneously heated, the energies of which are directed in one and the same way round the rectangle.

28. In the side, *c d*, there are two neutral points; one of which is exactly at the gate, or that point at which the metal was poured into the mould. I mention this circumstance more particularly, because I have observed that, when the point of heat is situated on different sides of the gate in any of these frames of bismuth, there are generally opposite currents elicited; or, in other words, the gate is generally a neutral point. The letter *o*, in Figs. 4, 5, and 6, denotes the gate in each.

29. *Experiments with the Rectangle B.*—By consulting Fig. 5, it will be observed that the greater part of the side, *a b*, is neutral. No deflection of the needle could be produced when the point of heat exceeded one inch from either of the angles, *a* or *b*.

30. The side, *c d*, has three neutral points, one of which is at the point, *o*, where the metal entered the mould. The two short sides, *b c* and *d a*, have each one neutral point. In *a d* it is at equal distances from the angles, *a* and *d*. In *b c* the neutral point is nearer to *c* than to *b*.

31. The opposite angles, *a* and *c*, are decidedly neutral points; but when the angles, *d* or *c*, were heated, very powerful currents were excited. The arrows on each side of the angle, *b*, are both directed the same way, and consequently would represent that angle of one uniform character; but it was found, by repeated trials, that there are two neutral points very close to the angle, *b* (but on different sides), and so close to each other, that it required a very fine pointed flame to heat one of those points without heating the other also.

32. *Experiments with the Rectangle C.*—There are three neutral points in each of the long sides of this rectangle, one of which is at *o*, the point where the metal entered the mould. In each of the short sides there is one neutral point, situated nearly at their centres, as is shown by Fig. 6.

33. The angles, *c* and *d*, are perfectly inactive when uniformly heated; but the angles, *a* and *b*, although represented by the arrows as causing conspiring currents on both sides of each angle, have, in fact, each of them two neutral points very close to each other; so that the needle will be deflected variously by heating different adjoining points about either of those angles.

34. When those parts of the rectangle which are opposite to the arrows, *i i*, are simultaneously heated, the energies of the conspiring currents become very powerful

indeed; and the needle may be driven round on its pivot through a whole circle by following it up with the side of the rectangle. The electric currents excited in this rectangle are more powerful than in either of the two former, particularly when heated opposite to the arrows, *i i*. The thermo-magnetic powers of A and B, however, when those rectangles were heated at two or more conspiring active points, would frequently deflect the needle over the arc of 30° or 40° by the first impulse.

35. The rectangle A was so exceedingly sensible by the slightest inequalities of temperature in its various parts, that the heat imparted by the finger and thumb by which it was held would excite an electric current of sufficient energy to deflect the needle 4° or 5°. Indeed, the temperature of the metal was very seldom so far equalized as to render its electric powers completely inert. The natural changes in the temperature of the atmosphere seem to be almost sufficient to perpetuate electric currents, without any artificial change whatever.

Fourth Class of Experiments.

36. As it had appeared from the preceding experiments that considerable thermo-magnetic action was elicited by bismuth, when cast into the shape of rectangular frames, I was desirous to ascertain if those powers were communicated to it by employing it in that particular shape; or if it would still display thermo-magnetic phenomena when cast into other forms. To set this question at rest, I cast several circular rings, or frames of bismuth. The exterior diameter of each ring was four inches, and the interior 3·5, leaving the metal 0·5 inch thick. They were cast with the plane of the mould horizontal, and open at the upper surface. The height of each ring, when in that position, was about 0·4 inch.

37. By applying the flame of a spirit lamp to various parts of each ring, and immediately presenting the metal to the compass needle in precisely the same way as had been done with the rectangle (11), several active points in each ring were soon discovered, the energies of which were continuous throughout every part of the circular frame, putting the whole circle into a state of thermo-magnetic activity. Several inactive, or *neutral points*, were also found in these rings, by heating which no perceptible influence was exercised on the needle by bringing the metal close to it. These results left no question remaining as to the magnetic power being innate and natural to the metal, and not communicated to it by its assuming any particular form. It must certainly be acknowledged, however, that the rings have never displayed the thermo-magnetic energies in so exalted a degree as I have observed in the rectangles; but this difference of energy may possibly be attributable to the difference in the extent of surface which can be exposed to the needle by having the bismuth in those

varieties of shapes; for, when the metal is in the shape of a circular ring, it is impossible to bring any more than a very small portion of its surface at any one time sufficiently near to the needle to examine the character of the Magnetism which it displays. With rectangles, or other straight-sided frames, it is quite otherwise; for, with those, the metal may not only be brought parallel and close to the needle for a considerable extent, but, if the same frame be sufficiently long, it can be made to operate on both poles of the needle at the same time—an advantage of considerable importance in experiments of this delicate character.*

38. Fig. 7 will illustrate the thermo-magnetic character of one of these rings, and will be sufficient, also, to give a tolerably good idea of the Thermo-Magnetism displayed by similar rings of bismuth generally—although some trifling difference as to the energy, number, and situation of the active points will frequently be found amongst them: for, in the present state of the inquiry, it is next to impossible to procure two exactly alike; nor is it easy to predict an active or an inactive point in a ring of bismuth which is symetrical in all its parts.

39. It will be observed, by contemplating Fig. 7, that the gate, *o*, is a neutral point. This I have also shown (28) is the case in rectangles; and indeed I believe that the gate will always be found in that state, into whatever form of open frames bismuth may be cast.

40. For the convenience of bringing a greater part of the edges of curvilinear frames as close as possible to the magnetic needle, I cast several into an elliptical form—the diameters of each of which were nearly as three to one. Every one of these frames became magnetic by heating at various points, in the manner already described; and the thermo-magnetic energies were as promptly displayed in every part of the curve by heating an individual point, as in any thermo-magnetic circuit whatever.

41. I must not omit to mention a very extraordinary circumstance which occurred whilst varying the experiments with one of those elliptical frames; because the same cause frequently produces very singular changes in the thermo-magnetic character of curvilinear and other frames of bismuth.

* Should it be asked why the multiplying Galvanometer was not resorted to in these delicate experiments, the answer would be, that that instrument, however valuable it may be for some purposes, is quite inapplicable to these inquiries—where every metal, excepting that under examination, should studiously be avoided, and on no account be permitted to enter the thermo-magnetic circuit. Errors frequently occur by employing the multiplier whilst examining the thermo-magnetic character of very small specimens of metals. Besides, the circuit in that instrument is frequently much too long to be penetrated by the feeble energies which are sometimes displayed in homogeneous metals, but which are easily detected in short circuits, through which they will pass with very great freedom. Doubts, also, regarding the correctness of the results, would necessarily have presented themselves had any other metal been permitted to enter the circuit; and with very great propriety indeed might every experiment have been questioned, had the Galvanometer, with its copper multiplying wire, mercurial cups, &c. been employed in an inquiry which professes for its object the contemplation of the Thermo-Magnetism of homogeneous metals alone. Moreover, these researches, as will presently be shown, have led to discoveries which could never have been made by the employment of the multiplying Galvanometer; and the character of several of the experiments is such as entirely to preclude the use of that instrument in their exhibition.

42. Having proceeded in the usual way (11), I succeeded in detecting several active parts in the ellipse. The directions of the various currents which were excited by heating those points are indicated by the arrows in Fig. 8—the arrow always pointing in the direction of the current, when any point was heated within its length. This done, I made a deep curved notch in the inner side of the rim, at the point *d*, Fig. 9, by means of the convex side of a half-round file. This small alteration in the edge of the frame caused such a wonderful difference in the activity of its Thermo-Magnetism as to surpass anything I had hitherto observed. The magnetic energies not only became very much exalted, but in several parts of the frame had completely changed their direction. When heated at the points, *o* or *f*, the deflection of the needle was at least three times greater than before the notch was made. When those two points were heated at the same time, the conspiring magnetic energies thus excited deflected the needle 50° at the first impulse; and, by changing the direction of the frame to promote the deflections, the needle was soon made to sweep an arc of more than 200°; and, by following the oscillating needle with the edge of the frame, the former was driven by the thermo-magnetic forces of the latter several times quite round the whole circle.*

Remarks on the preceding Experiments.

43. It would be needless (indeed, almost endless) to describe all the peculiarities which have attended the numerous repetitions of the preceding experiments; and the variety of circumstances under which I have cast rectangular, triangular, and curvilinear frames of bismuth—as the phenomena already described, with a few summary remarks, will pourtray their general character.

I am not certain that any peculiar property is communicated to bismuth by the magnetic process (23), or by the position of the metal, as regards the cardinal points,

* My motive for making this deep notch in the side of the frame, was that of checking the progress of the heat in one direction more than another, when applied to either side of it—an object which I had in view from the commencement of these experiments, and which I had in vain attempted to accomplish by means of angular points (13); and, from frequent disappointments by the apparently fortuitous results in the preceding experiments, and many others which I have not mentioned, I had lost all hope of arriving at anything like interposition in any other way than that of modifying the crystallization of the metal. The results of this experiment, however, were very singular; and I have since found that similar results frequently happen from the same cause. This, however, is not always the case: it takes place most frequently in those frames which, whilst whole, are not very active; and a neutral point ought to be selected for the situation of the notch. I have frequently reduced particular parts of circular rims of bismuth with a hot iron, and sometimes with the flame of a spirit lamp, instead of a file, and with similar results.

There is another method of exhibiting a very remarkable and nearly-allied property of Thermo-Magnetism, much more decidedly than by that of filing—by experiments which do not properly belong to this class, although they were suggested by the results of the experiment last described; besides, there are impediments of which I must warn the reader before I can possibly describe the method of arriving at anything like success. The circumstances, however, which I have called impediments, emanate from the display of a very interesting class of phenomena, which I shall presently describe.

whilst casting. I have frequently cast rectangles under these circumstances, but have not found more regularity in the display of their Thermo-Magnetism than in those which were cast under circumstances quite different.

In general, small and well-formed rectangles (17), Fig. 3, operate with much greater regularity, as regards the angles, than those which are of larger dimensions, Figs. 4, 5, 6. But it frequently happens that the energies of the latter are much superior to those of the former.

The currents, more frequently than otherwise, proceed in opposite directions, when excited on different sides of an angle ; and in those cases where it happens otherwise, the neutral point is situated almost close to the angle.

The gate is invariably a *neutral point* when the mould is not very hot at the time of casting ; and more frequently than otherwise in castings generally.

The thermo-magnetic character of frames of bismuth, whether *angular* or *curvilinear* may be considerably modified by removing, or partially removing, crystalline groups, or by altering their forms (41, 42, and note).

Every part of a continuous ring or rectangular frame of bismuth becomes magnetic by heating the smallest possible point.

The phenomena, generally, are sufficiently striking to be exhibited in a lecture room, and to be observed by the most distant auditor, provided the needle be neutralized. Two or more rectangular frames may be selected, and properly adjusted, side by side, so that their combined energies may be made to operate simultaneously on a very large needle. In this way, the experiments may be made on a very extensive scale.

Fifth Class of Experiments.

44. The experiments which I am now about to introduce present phenomena, perhaps somewhat less complicated, though by no means less curious, than those I have already described.

I had observed, whilst experimenting with a rectangular frame of bismuth, of large dimensions, that the needle would sometimes be deflected in one direction and sometimes in the other, even when the point of heat was not varied. Struck by this unusual phenomenon, I proceeded to examine it with some degree of minuteness, and with an intention of ascertaining its cause.

45. Before noticing this apparent anomaly, I had constantly held the plane of each rectangle in the plane of the magnetic meridian, and with its lower edge as close as possible to the needle—a position which I considered as the most likely to obtain true results ; because, when so placed, the upper edge of the frame, in consequence of its great distance, could not affect the deflections of the needle produced by the thermo-magnetic forces in the lower side. This position of the frame would certainly have

been better than any other for ascertaining the direction of single electric currents, or those thermo-magnetic forces which are circumfused in the apparatus of Seebeck, and other similar combinations; although a slight degree of inclination, either to the east or west, with such compound apparatus, would not very materially have affected the results. I soon became convinced, however, that the anomaly which I had noticed proceeded entirely from that cause; for if the plane of the rectangle inclined to the east, the needle would be deflected in a contrary direction to that which it assumed, by inclining the plane of the rectangle to the west. By several trials it was found that, when the plane of the frame became nearly horizontal eastwards, still keeping the end north, a greater deflection was obtained than by holding it at any other angle on that side of the meridian. And by placing it in a similar position to the west of the meridian, the greatest deflection was again produced in the opposite direction to the former.

46. Considering this as one step, at least, towards the discovery of the cause of this novel phenomenon, I proceeded to examine the opposite side of the frame, which I supposed might possibly present similar effects. For this purpose I heated one of the points in that side which, by previous trials, was found to be very active, and then placed it directly over the magnetic needle, keeping the plane of the frame in the magnetic meridian, and the same end north as had been in that position whilst experimenting with the other side. The needle was deflected in precisely the same direction as I had always found it to be by a similar position of the frame, and with the same point of heat. I afterwards inclined the plane of the frame—sometimes eastward, at others to the west—but in no instance could I obtain the least indication of results similar to those which I had noticed whilst the opposite side of the frame was nearest to the needle. Thinking that the different active points might possibly operate under different laws, I next heated the point which had before presented the extraordinary phenomenon, still keeping the other side nearest to the needle; but nothing remarkable was noticed by this variation of the experiment. The needle continned to be deflected in one uniform direction, whatever inclination was given to the plane of the rectangle. I tried the other sides of the rectangle in precisely the same way, but obtained no unusual results. It now appeared evident that one side of the frame was endued with peculiar properties which the other sides could not be made to exhibit, which soon proved to be the fact.

47. When this side was heated at one particular point, two distinct electric currents were called forth; one of which may be distinguished by the name of *general* current, because it pervaded every part of the metallic frame; the other current was perfectly *local*, and could be traced only to a short distance from the point of heat. It never reached further than the angle, and returned into itself on the opposite face of the solid prism which formed that side of the frame.

48. From this singular result, it appeared likely that some inaccuracy might possibly have occurred in the conclusions I had already drawn from the former experiments. Fortunately the rectangles were not broken, and I had an opportunity of examining them again; but it seems they were of too small dimensions to produce *local* currents, as I found them to operate exactly as at first. On trying one of the circular rings, however, another curious fact was discovered, which at that time was not a little surprising. A reference to Fig. 10 will assist the illustration of this singular property discovered in the ring of bismuth.

49. When the outside of the ring, *b*, was held for a few moments in the point of a well defined flame of a spirit lamp, the electric current in every part of the ring was in the direction of the exterior arrow; but when the inner side of the rim opposite to *b* was similarly heated, the current proceeded in the contrary direction, as indicated by the interior arrow. And so directly opposite, and close to each other, were these two active points, that a transverse section of the metal at the point, *b*, would have embraced them both. I have since that time observed active points similarly situated in other pieces of bismuth.

50. *Experiments with a Solid Prisim of Bismuth.*—That side of the rectangle (47) which had exhibited local electric currents was now cut from the frame, for the purpose of ascertaining whether phenomena similar to those described (47) would be exhibited by this prism when examined as a distinct individual mass. Fig. 11 will represent this prism, or bar of bismuth. When heated at *b*, the former active point, the bar became highly magnetic, displaying those energies in a very exalted degree. The electric current was traced from the point of heat towards the end, *c*, along that side of the prism which in the figure is hid from view. It proceeded across the end of *c*, as shown by the curved arrow, and returned to the heated point along the front face of the prism; so that when heated, if the front face, *a b c*, were placed over, and parallel to the needle and the end, *c*, north, the north end of the needle would be deflected towards the east; and by simply turning the prism the other side up, the needle would be deflected towards the west. By thus turning the bar, at suitable times to promote the deflections, the needle could be made to sweep an arc of 90°.

51. The other two faces of the prism were nearly neutral, each of them partaking of the opposing currents in the active sides.

52. The bar was afterwards heated at various parts of its surface, and the direction and energy of the currents ascertained by the deflections of the compass needle. It was ultimately found that when the whole of the end, *c*, was uniformly heated, the most powerful current was excited: its course was still in the same faces of the prism, but its direction was reversed.

53. When the end, *a*, was uniformly heated, no Thermo-Magnetism was elicited, until, as the heat advanced in the bar, the temperature became disturbed at the point, *b*.

This accomplished, those powers were again roused from quiescence, and displayed phenomena precisely of the character as when the bar was heated at the point, *b*, only.

54. The neutral end, *a*, was now cut off at the point, *b*; and the remaining bar displayed Thermo-Magnetism to whatever end heat was applied. The currents still passed over the same faces; and, as regards the point of heat, uniformly set out over the same face and returned by the other. Hence the apparent contrariety in its direction when the bar was heated at different ends. These results led to further experiments with bars, and other forms of solid homogeneous metallic masses—some of which have developed very curious phenomena, which appear to observe a uniformity in the laws of their exhibition.

55. *Experiments with a Cylindrical Bar of Antimony.*—This bar was eight inches long and 0·75 inch in diameter. It was very far from having a uniform surface, being much more cavernous on one side than the other, from air-bubbles whilst casting. It was heated at various points of its surface by a very fine pointed flame of a spirit lamp; and its Magnetism, thus excited, was traced by its action on the compass needle. It was ultimately discovered, that whatever point near to the extremities of the cylinder was selected for the point of heat, the electric current invariably flowed over the same parts of its surface; and, when either of the ends was uniformly heated, whilst the other was kept at the temperature of the atmosphere, the bar became highly magnetic—exhibiting phenomena similar to those already spoken of as appertaining to the prism of bismuth (54). The electric current constantly passed over the dense and opposite cavernous sides, whilst the intermediate longitudinal lines were nearly neutral.

56. Fig. 12 will give a good idea of the direction of the electric currents excited in this bar of antimony, when one of its ends was uniformly heated. The cylinder is supposed to be divided into halves in the plane of its axis, and terminating on each side by the dense and cavernous parts of its surface. The two halves are placed edge to edge, with their convex sides upwards. The dotted line in each half will represent the neutral line, or that longitudinal line on each side of the bar, which, when placed parallel over the needle, no deflection was produced. The active lines are *c d* and *a b*, *a b*: the two latter correspond to each other when the halves of the cylinder are replaced or brought together. It is to be observed, however, that the thermo-magnetic energies of the cylinder were not confined to two longitudinal lines, for every part of the surface near to the heated end was more or less magnetic; but, in consequence of the recurved manner in which the electric currents flowed over the surface, there were necessarily two longitudinal lines more active than any other part. These lines passed through the dense and opposite cavernous sides, and may be termed the lines of *greatest energy*. The *neutral* lines were also a consequence of the recurved flow of the electric currents, by intersecting them at right angles; and, as those inter-

sections were in a series of points nearly parallel to the axis, those neutral lines were nearly parallel to the axis also.

57. When the other end of the cylindrical bar was heated, the lines of *greatest energy* were still on the same parts of the surface, but the electric currents flowed in an opposite direction to the former; so that, to whatever end of the bar heat was applied, the current uniformly proceeded from the heated end along the dense side, *c d*, and returned over the opposite or cavernous part of the surface, *a b*, *a b*.

58. The thermo-magnetic energies never reached to the cold end of the bar, but returned to the point of heat in directions indicated by the arrows, and at no great distance from the heated end. The same laws hold good in all cylindrical bars of antimony of small dimensions, which are not of a uniform density on every side of the axis. I have broken several into fragments, for the purpose of examining their internal structure, and have always observed that, when they display phenomena similar to those last described, their density is not uniform; and the side of the cylinder which, in a transverse fracture, will exhibit the most compact texture may generally be predicted by an observance of the thermo-magnetic phenomena which it will display whilst whole.

59. Cylindrical bars of antimony, of a uniform density on every side of the axis, and more than two inches diameter, display thermo-magnetic phenomena with very great precision, and a rigid observance of certain laws.

60. When a bar of this description, and six or eight inches long, has been cast in a vertical mould of sand, let its ends be struck off with a sharp-edged hammer, making the sections transverse and not ragged. Apply the flame of a fierce spirit lamp for a few moments to the convex surface, close to one end of the cylinder, and immediately place the heated side downwards, over and parallel to a delicately-suspended compass needle. If the heated end of the bar be placed north, the north end of the needle will be deflected *eastward*—showing that the electric current by which it is deflected is flowing along the lower side of the bar, from the point of heat towards the cold end.

61. Let the same point be again heated in a similar manner, and again place the cylinder over the needle, with its heated end north, but with the point of heat upwards. The north end of the needle in this case will be deflected towards the west, or in the opposite direction to that which it assumed in the former experiment. The deflection of the needle indicates the electric current to be flowing in the cold side of the cylinder, from south to north, or in the opposite direction to that in which it flows in the heated side.

62. Heat the same point again; but, instead of placing the heated or opposite side of the cylinder over the needle, as in the former cases, place one of those parts of the surface over it, which is about 90° from the point of heat, still keeping the cylinder

parallel to the needle. In this position, the latter is scarcely affected; and, by a few trials, a line will be found on the surface of the cylinder, and nearly parallel to its axis, which has no action whatever on the needle. This is one of the *neutral* lines; and, by a few trials on the other side of the point of heat, another *neutral* line will be discovered. These lines are generally at about 90° from a line drawn from the point of heat to the other end of the bar, and parallel to the axis.

63. This latter line is one of the lines of greatest energy; the corresponding line of greatest energy being parallel, but on the opposite side of the cylinder (56.)

64. Similar phenomena will be displayed by making any other part of the convex surface near to the ends of the cylinder the point of heat. The current uniformly flows over the surface, on the heated side, from the point of heat, expands into two distinct tides which sweep the surface of the metal, and re-uniting on the opposite side, recurves into itself at the heated point of the cylinder.

65. The general distribution of the electric force on the surface of the cylinder, by heating it as directed (60, 61, 62,) will be pretty accurately indicated by the arrows in Fig. 13. The cylinder is supposed to be divided into halves, and its convex sides upwards, as in Fig. 12 (56.) The straight arrows indicate the lines of greatest energy, and the edges, *a b*, *a b*, which coincide when the halves are replaced, are in one of the *neutral* lines: the other *neutral* line is *c d* in the centre of the figure.

66. The thermo-magnetic energies can hardly ever be traced more than four inches from the point of heat; they are, however, excited to a certain extent by the slightest disturbance of temperature near to either of the ends of the cylinder.

67. When any point is suddenly made pretty hot, without elevating the temperature of the opposite side, which can easily be done when a cylinder is employed of more than two inches diameter, the electric force is very considerable, and will deflect the needle to an angle of 20° or 30°; and by dexterously turning is the other side up before the returning needle arrives at the magnetic meridian, another impulse is given, and the angle increased on the other side. Two or three turns of the cylinder in this way, will cause the needle to sweep a considerable arc; but the arc over which the needle passes will be very much increased if the needle be followed up by the active sides of the cylinder, still keeping the one parallel to the other.

Remarks on the preceding Experiment.

68. There is a peculiarity in the phenomena displayed in this experiment, which has not been observed in any of the rest. When a cylinder of antimony is cavernous on one side, I have shown (57) that the electric current invariably flows over the same parts of the surface; but in cylinders of uniform density on every side of the

axis, the law of thermo-magnetic action is very different, and the route of the electric current over the surface of the metal entirely depends upon the situation of the point of heat.

69. When a cylindrical bar of antimony is uniformly dense on every side of its axis, it will invariably present a regular crystalline form at every transverse fracture. The general contour of the section is that of a series of exceedingly thin, concentric crystalline laminæ, of which the whole face of the fracture seems to be composed from the centre to the surface of the cylinder. Aided by a magnifier, the eye is enabled to trace apparent radiating veins, which, by close inspection, are observed to separate the laminæ into distinct parcels or tall narrow bundles, with their edges inclined to each other at various angles, both salient and re-entering: and the apparent veins, which are frequently nothing more than an angle at which two bundles of laminæ unite, give to the fracture a beautiful glittering appearance. Some of those radiant veins, however, are absolutely the flat facets of laminæ, or more frequently the sloping edges of bundles of them, which have a brilliancy far superior to any other part of the fracture. The general position of the laminæ, however, is, that their flat surfaces are presented to the axis of the cylinder; and, although there are certainly objections to this position being uniformly determined by the crystalline laminæ, because of several of the piles or bundles being posited at various angles, yet the major part of those piles are absolutely set in that position, and not a single crystalline film has its plane determined at right angles to the axis of the metal. Hence it is that the edges of the greatest part of the bundle of laminæ are presented to view at every transverse fracture, and may be compared to tall narrow bundles of thin metallic leaves, or slips of paper, placed round a central nucleus, with one of their narrow ends presented to the centre, and the other towards the surface of the cylinder; which position, together with others which some of those bundles assume, give to them the appearance of radii, with various degrees of splendour. Fig. 14 will assist in giving an idea of the general disposition of the strata of crystals in a transverse section of an uniformly dense cylinder of antimony.

70. If the sharp edge of a hammer be applied in the direction of the axis, the cylinder may be completely dissected from its surface to its centre; or the crystalline layers may be peeled off, one after another with very great accuracy, as far as the dissection is required to be carried on. When a cylinder of antimony is thus disrobed, it presents an exceedingly beautiful appearance: the refulgent facets of its crystals are exposed to view, which stud its surface as if it were decked in a most brilliant coat of mail; whilst the multitude of spangles which those facets display are now seen to be disposed in the crystalline arrangement already described.

71. Assuming, then, that the general crystalline arrangement is that of concentric laminæ, two hypotheses may, perhaps, present themselves for an explanation of the

thermo-magnetic phenomena elicited in an uniform cylindrical bar of antimony, one of which, it appears to me, will ultimately be found to be the true theory.

72. First, then, it may be supposed that the opposite faces of each metallic film are in different states of Electricity, or at least that they have different thermo-magnetic qualities. If it could be satisfactorily proved that this were the case, their concentric arrangement would reconcile the phenomena to all those which are displayed by the juxta-position of any pair, or series of pairs, of dissimilar metallic plates, and each bundle of films would become an electric column. In that case, the thermo-magnetic character of the inner surface of each film would be to its outer surface as bismuth is to antimony; for the current in a pair of those metals flows, through the point of heat, from the former to the latter, and the rest of the circuit answers no other purpose than that of a conductor. When the point of heat is close to the edge of a transverse fracture of a cylinder of antimony, two, or a very few more, of these plates or crystalline films may possibly be the only part excited, and the rest of the bar assume the character of a conductor only; in which case the current would flow, at the point of heat, across the films from the internal to the external parts of the cylinder—the direction which experiment discovers it to proceed in: besides, it is possible that the crystalline laminæ may individually have different electric powers.

73. The other hypothesis supposes, that as the crystalline strata are only in juxta-position, and not very firmly united, it is possible that the heat applied at any point on the surface of the cylinder, would meet greater obstacles in its progress whilst passing from film to film, than any which it would fall in with whilst flowing over the surface of those films, or over the general surface of the metal: and as heat is well known to affect electrical phenomena generally, and as it is the exciting agent in this particular class, it may be supposed that, by its travelling at different rates in those directions, the electric powers of the metal may also be put into motion, and assume certain uniform directions, as regards the directions in which the heat flows, with the *greatest* and *least* facility.*

74. *Experiments with Solid Cones of Antimony.*—When a solid cone of antimony, uniformly dense on every side of its axis, is made the subject of experiment, the surface near to its base displays thermo-magnetic phenomena of precisely the same character as those which have been described in the experiments with a cylinder (60, 61, 62.)

75. The cones which I have employed were 4·5 inches high, and the diameter of the base 2·25 inches.

76. When any of these cones were heated at any point of the side near to the base, the current uniformly proceeded from the point of heat over the surface towards the

* These hypotheses are offered merely as conjectures, without any intention of insisting on either of them, until experiment affords more data in their favour. If I mistake not, however, some of those which I have yet to describe will bear directly on the subject.

apex, and returned on the opposite part of the surface to the base. This was the direction of the lines of greatest energy, but, like the cylinder, the surface of the cone becomes generally thermo-magnetic by this process, and the direction of its forces are easily traced by the compass needle.

Fig. 15 will represent the surface of a cone of antimony in a state of thermo-magnetic action: the cone is supposed to be divided into halves from its apex to its base, and in the plane of the neutral lines. The same explanation will apply to this figure as to the cylinder, Fig. 13 (65.)

78. It is not necessary that the point of heat be exactly in the edge of the base to produce the greatest effect, for the direction of the electric force is still the same, and quite as energetic, when the point of heat is at some short distance from the base. Neither is it necessary that any point be made very hot, unless it can be done very suddenly; for the powers excited are decisively exhibited when the selected point of heat is held only for a few moments in the apex of the flame of the spirit lamp, and the cone immediately applied to the compass needle, before the heat has time to spread, to any great extent, over the conical surface.

79. When the apex of the cone of antimony is heated, the electric force is exceedingly feeble, and its direction quite uncertain. In general, the thermo-magnetic forces displayed, by heating any point nearer to the apex than to the base, are comparatively insignificant, and their directions not easily predicted.

80. A cone of antimony, which had exhibited the phenomena already described, was cut in two by a saw, at about 1·5 inches from the apex, and parallel to the base. The small cone operated precisely as the original one, of which it was a part; but the energies were by no means so powerful.

81. The frustum presented phenomena as if it had been a complete cylinder, and the electric currents were as decidedly traced when the point of heat was near to the section as when it was near to the base.

82. When cylindrical bars or cones of bismuth are experimented with in the manner I have described with antimony in those shapes, the thermo-magnetic phenomena are precisely of the same character, and are regulated by the same laws; so that whatever phenomena be displayed by the one metal will also be displayed by the other, provided the cylinders or cones be well cast, and of uniform density on every side of the axis.

83. In bismuth, however, it sometimes happens that, in consequence of an irregularity of crystallization, which it is prone to assume, there will be one point, and sometimes two, which, when heated, will display thermo-magnetic phenomena very different to those I have before spoken of; but these are irregularities which have nothing to do with the general character of the phenomena, and but seldom occur.

84. *Observations.*—Whatever peculiarities there may be in the crystallization of antimony and bismuth when in masses of other forms, they exhibit arrangements exceedingly similar to each other when cast into cylinders, which are regularly and uniformly cooled on every side; and there is so little difference in the general aspect of a transverse fracture of the two metals that, were it not for the difference of colour, it would require some practice to distinguish the one from the other. From this circumstance it appears highly probable that the same cause, whatever it may be, is the fountain of the Thermo-Magnetism in both metals.

85. It has been intimated to me, by some very scientific gentlemen, that impurities in the metal may possibly be the cause of all the thermo-magnetic phenomena, which I have attributed to homogeneous bodies; and I must confess that, for some time, I had entertained a similar opinion: experience and observation, however, by no means sanction the concession. Some other cause than that of impurity in the metal is unquestionably in active operation; and to some other cause we must direct our attention before we can accomplish an explanation of the phenomena in question. A very small portion of tin added to bismuth, not only dispossesses it of its magnificent crystalline ramifications, but also of the superlative display of its natural innate Thermo-Magnetism: moreover, that small morsel of tin not only paralyses the Thermo-Magnetism natural to bismuth as a homogeneous metal, but absolutely transfers its thermo-magnetic character, as regards other metals, from one extremity of the range to the other; so that if pure bismuth be regarded as the most positive metal, its alloy with tin will be the most negative substance, either simple or compound, with which we are acquainted; and antimony, which has hitherto claimed the negative extremity of the range, is highly positive to this simple alloy.

86. The Thermo-Magnetism natural to antimony becomes completely stagnated when mixed with tin or lead, and the crystals of the metal become insignificant shapeless specks. Zinc also, which, when in larger masses, displays its innate Thermo-Magnetism in a degree superior to any other metal, except antimony and bismuth, becomes comparatively inert by a mixture of tin or lead.

And what, perhaps, may appear a more convincing fact than all the rest is, that antimony and zinc, which separately, as homogeneous bodies, display fine crystalline forms, and also active Thermo-Magnetism, will, if mixed together as an alloy, become robbed of those distinguished characters at once, and the resulting metal appears as compact as the finest steel.

87. Whatever may be the notions entertained, as regards the mass or quantity of metal employed in heterogeneous thermo-magnetic combinations, I find that in the display of the thermo-magnetic phenomena of homogeneous bodies, the quantity employed is an essential consideration; for in several of the metals, although no trace of Thermo-Magnetism can be detected in small pieces, its powers are promptly

developed in masses of considerable dimensions, and the laws of its phenomena may be determined with precision. Zinc, when in large masses, displays thermo-magnetic phenomena in a very exalted degree; but in small pieces hardly any trace of that power is to be found. Copper is a still more striking instance of the superior thermo-magnetic powers of large masses. Those powers could not be detected in a few ounces of the metal; but in a mass, weighing sixty or seventy pounds, they would become very conspicuous. But a mass of copper of a hundred weight, however heated, would not deflect a needle half so far as it would be deflected by a single ounce of bismuth or antimony. Yet, insignificant as these powers are in some bodies, I have succeeded in detecting them in every metal of which I had a sufficient quantity at command; and I have no doubt that they may be discovered in all the metals.

Sixth Class of Experiments.

88. *Experiments with Irregular Masses of Antimony.*—The object of these experiments was that of ascertaining if the same laws of thermo-magnetic action, as regards the crystalline arrangement of the metal and point of heat, as those which were developed in the experiments with cylinders and cones of antimony, could be traced in masses of an irregular figure, by making the point of heat in various parts of those fine smooth extensive faces of crystalline laminæ, which are to be met with only in fractures of large masses which have been very gradually cooled from fusion.

89. The experiments were made by heating separately particular points in those lamellated faces, and then tracing the direction of the electric currents by expeditiously applying the antimony to a magnetic needle, and noting minutely the character of the deflection; and it appears, from the uniformity of the results of a considerable number of experiments on various pieces, that the thermo-magnetic phenomena elicited in irregular masses, have precisely the same relation to the position of the metallic films, and point of heat, as those displayed by cylinders and other regular forms of antimony.

90. It will not be necessary to enter into a detailed account of the several experiments which were instituted for this inquiry, as a description of those which were made on one of the irregular pieces, and of the resulting phenomena, will be sufficient to illustrate the whole. And I have every reason to believe, that the *same laws* which govern these phenomena, will be found to appertain to all similar crystalline arrangements of antimony; that they will uniformly be developed under similar circumstances, and consequently that they are intimately related to the crystallization of the metal.

91. The piece of antimony on which these experiments were made, weighed about three pounds; it was separated by the blow of a hammer from a large mass, and the

fracture exposed a smooth triangular face of parallel crystalline plates, without presenting any intersecting edges of metallic laminæ whatever. This triangular face will be represented by Figs. 16, 17, 18; and the arrows in those figures will show the directions of the electric currents, as indicated by the deflections of the magnetic needle, when the point of heat was near to the angles, *a b c*, respectively.

92. When the spirit lamp was held for a few moments at the angle, *a*, still keeping the point of the flame on the face of the fracture, the electric streams were diffused over every part of that surface *from* the point of heat towards the opposite edge, as shown by the directions of the arrows, Fig. 16. Comparatively strong currents were detected in the edges, *a b* and *a c;* but, in consequence of the general flow of these currents being nearly at right angles to the edge, *b c*, no magnetic force could be detected when that side was held over and parallel to the needle. On leaving the face, *a b c*, the electric tide swept the general surface of the metal, flowing in various directions, and returning by numerous windings to the point of heat. By this distribution, and consequent attenuated state of the electric force, the thermo-magnetic energies were comparatively very feeble on every part of the metal, excepting the face, *a b c*, on which alone they were displayed with promptitude and regularity.

93. When the point of heat was at *b*, the whole of the triangular face became again magnetic, displaying phenomena of precisely the same character as those which had been elicited when the point, *a*, was excited; and the distribution of the electric forces had again a decided reference to the point of heat—emanating therefrom, and flowing with as great a uniformity over the surface of the fracture as if it had been a conductor from the copper to the zinc of a single Galvanic pair. The arrows in Fig. 17 will indicate the distribution of the electric force over the surface of the lamellated fracture when the point of heat was at *b*.

94. When the fracture was heated at *c*, the thermo-magnetic phenomena were again displayed with very nice precision and uniformity on that particular face of the metal; whilst, on the other parts of the surface, they were confused and irregular—showing that the electric forces on those parts were dispersed in various directions, and enfeebled by their separation, or by their returning to the point of heat, through the body or general mass of the metal. Fig. 18 will show the direction of the electric tide on the face, *a b c*, when the point, *c*, was excited by the flame of a spirit lamp.

Remarks.

95. The uniformity displayed in the results of the preceding class of experiments confers on them a very interesting character in these investigations. In connection with those on regular masses, these experiments establish a very important point, by

exhibiting, in the most striking and satisfactory manner, an intimate connection between the crystalline arrangement of the metal and the distribution of the electric powers by heat; for, to whatever point in the flat lamellated face of this system or group of parallel scales heat was applied, the electric forces were directed over the planes of the laminæ *from* the heated point; and, having traversed the general surface of the metal, returned to *that point* again, *across the edges* of the films, in precisely the same manner as in the experiments with solid cones and cylinders—a circumstance highly demonstrative that the thermo-magnetic forces in both sets of experiments have the same specific origin, and are actuated by the same cause. The fountain of all the phenomena appears to be in the crystalline arrangement of the metal, and the direction of the electric and magnetic forces to be referable to the point of heat.

96. It very often happens that fractures, such as have been described (88, 91,) are bordered on some of their sides with piles or groups of laminæ, unfavourably situated for experiments of this kind—presenting their thin edges, instead of their planes, in the face of the fracture. When, however, the method of experimenting becomes known, these trifling inconveniences are not of much consequence to the uniformity of the Thermo-Magnetism displayed by the smooth part of the fracture under examination.

97. In the first place, the flat scaly surface on which the experiments are to be made ought to be as extensive as possible—at least, two inches across; if larger, the better. Should any side of this face present groups of the thin edges of laminæ, they may be easily removed, either by the saw or by the hammer: if those groups be not very extensive, their removal will not be necessary.

98. The principal circumstance next to be observed is, that the flame of the spirit lamp does not touch those unfavourable crystals. The selected point of heat must always be on some part of the flat lamellated face under examination, and near to some angle. A momentary heat must suffice, and the plane immediately and dexterously applied to the magnetic needle—the deflections of which will unerringly indicate the electric current to be flowing over that surface *from* the heated point to the opposite side.

99. I have succeeded in discovering a method of forming square bars or prisms of antimony, which observe a rigid uniformity in the display of thermo-magnetic phenomena, by heating them either partially or equably at one end only; and I now find that I can predict with certainty the magnetic character of any side of the bar, by paying attention to certain circumstances connected with its casting. I have cast several sets of square bars, of a uniform size and figure, under precisely the same circumstances, and have never yet found one single bar to deviate from the general law. One of those sets consisted of fifteen bars, all of which observed the same laws

of thermo-magnetic action. I have, however, in vain tried to obtain them of a uniform power, the Thermo-Magnetism of some of them being much more energetic than that of others. This circumstance, which I hope soon to obviate, and some others which I find associated with the display of their thermo-magnetic phenomena, but which I have not yet had time to investigate, prevents my giving a description, in this place, of the circumstances under which I have hitherto cast these prisms of antimony—the thermo-magnetic character of which can easily be predicted before the metal enters the mould.*

100. In general, these bars possess a considerable degree of power as thermo-magnets; and when four, or more of them, are properly combined, their conspiring energies on the magnetic needle may be very satisfactorily exhibited to every auditor in the most spacious lecture room.

101. I have also been enabled to cast discs of antimony, which do not vary from each other in the character of their thermo-magnetic qualities. I have not, however, as yet, had time to investigate the whole of the circumstances which I suspect to be connected with the communication of that power to the metal; and, therefore, beg permission to reserve the detail of the experiments till another opportunity. I mention them in this place merely as facts, which I can at any time repeat. I will further observe, however, that I am of opinion that the Thermo-Magnetism displayed in the prisms and discs already noticed, may be traced to the same source as that displayed in other forms of antimony—that is, to the crystalline arrangement of the metal; and that Electricity is intimately associated with the process of crystallization generally. This opinion is highly favoured by the well-known fact of electro-polarity being exhibited in the tourmaline and some of the crystallized gems; and, as regards the metals, I imagine that the experiments and observations I have hitherto detailed are amply demonstrative of the connection in that class of homogeneous bodies. And I am inclined to believe that future labours in this curious philosophical field of research will ultimately establish crystallography amongst those interesting sciences, which are subordinate branches, and obedient to the laws, of Electricity.

103. There are, however, thermo-magnetic phenomena displayed by homogeneous metals, when experimented with in certain forms, which do not appear to be very reconcilable to the hypothesis of *electro-crystallography*. They seem to depend upon some other cause than any which that hypothesis embraces; and as they are exhibited under different circumstances to any which have yet been noticed, the experiments by which they are elicited will require to be described as a distinct class.

* It is next to impossible to cast bars of antimony, of considerable dimensions, which will not exhibit magnetic phenomena by heat: indeed, bars of almost any size, or masses of any figure whatever, whether regular or irregular, display those powers more or less. It, however, requires considerable attention to obtain several pieces of antimony which will observe a uniformity of thermo-magnetic action.

Seventh Class of Experiments.

104. Notwithstanding the opinions which have been set forth to show that thermo-magnetic energies are not exalted in *combinations* of metals, by employing them of large dimensions; and that a pair of particles, however small, or wires exceedingly thin, will develop the same extent of power as two bars of considerable dimensions—I was led to imagine that the same law might probably not extend to the innate Magnetism displayed in homogeneous metals by heat. My inquiries were, therefore, directed to large masses of those metals in which, whilst experimented on in small pieces, I was unable to discover the least trace of this extraordinary power; and the results were such as to answer my anticipation in the most ample and satisfactory manner.

105. *Experiments with large Masses of Zinc.*—The first piece of zinc in which I detected thermo-magnetic action was a rectangular cake, or flag, which had neither been rolled nor hammered. It was about 14 inches long, 8 inches broad, and 0·75 inch thick, and weighed about 17 pounds. This mass of zinc, when heated at one corner only, displayed magnetic powers in a very exalted degree, and would deflect a compass needle, on which the Magnetism of the earth was not neutralized, 20° by the first impulse, when one of its edges was held in the magnetic meridian, and close to the glass cover of the instrument; but, in consequence of a fracture in one of its edges, the thermo-magnetic phenomena were not so nicely regulated in this piece as I have found them to be in other masses of zinc, which are uniformly sound on every side. I will therefore describe experiments which were made on a whole sound flag of zinc, weighing forty-two pounds, two feet long, 8·5 inches broad, and about one inch thick.

106. The thermo-magnetic phenomena were promptly and uniformly displayed by this mass of zinc, and were precisely of the same character as those which I have observed in experiments with all similar masses that I have yet examined. They have an evident reference to the point of heat; and, I believe, they may be taken as a general standard for those which would be developed by all similar masses when operated on by the same process.

107. The experiments were made by heating one corner of this mass of zinc in a common fire, and keeping the other parts of the metal as cool as possible. When thus heated, the mass was held in various directions over the magnetic needle, the deflections of which were taken as indications for the directions of the electric currents. In this manner the thermo-magnetic powers of the zinc were ascertained, whilst it was partially heated at its several angles in succession.

108. If *a b c d*, Fig. 19, be permitted to represent one of the flat faces of the zinc plate, then the arrows in that figure will give a tolerable representation of the direc-

tion of the electric forces, as indicated by the deflections of the compass needle when the point of heat was at the angle, *d*. By contemplating this figure, it will be observed that the electric forces are projected, as it were, *from* the heated angle into the body or field of the mass, over which they become generally diffused; but, separating and expanding in different directions, they sweep the surface of the metal in recurving tides towards its edges, by which routes they again return to the heated point.

109. The straight arrows in Fig. 19 would seem to indicate that the electric forces in those parts of the metal were directed in right lines, which is not strictly correct. Those arrows are drawn to show the lines of *greatest energy*, or those parts of the metallic surface which, when presented to the needle, produce the greatest deflections, and are the determined resultants of the numerous curvilinear forces which are in active play during the transient disturbance of temperature in the metal.

110. It will appear evident, by inspecting the figure, that on the surface of the rectangular mass there would necessarily be two *neutral* lines, one on each side of the diagonal arrow, which would be determined at right angles to the aggregate recurving electric forces, and may be represented by the dotted lines, *d n*, *d n*. These lines are those in which a magnetic needle, unsolicited by any other force, would arrange itself, and were discovered by its uniform repose whilst situated close to those parts of the metal. The situations of the neutral lines, however, are not constantly the same; for as they are determined by the direction of the electric forces, and those forces again by the distribution of heat, the situations of the dotted lines, *d n*, *d n*, will also vary with the circumstances of heat.

111. When one of the ends, *a d*, Fig. 20, of the rectangular mass is uniformly heated, the distribution of the electric forces will be indicated by the arrows in that figure. Here, again, it will be observed that the electric forces are projected *from* the heated end into the area of the plate, and, by recurving sweeps, return to that end again along the parallel margins of the metal. In this case the *neutral* lines, and lines of *greatest energy*, are parallel to each other, and also parallel to the sides, *a b d c*, of the rectangular plane.

112. As a similar distribution of the electric forces to that represented by Fig. 19 is uniformly elicited by heating any of the angles separately, the same system of arrows will serve to illustrate that distribution, to whatever angle heat may be applied. If, for instance, the angle, *a*, were to be heated, the points of the straight arrows, *d c*, *d a*, would then be directed to *a*, or towards the point of heat; whilst the feathered end of the former would be directed towards *b*, and that of the latter towards the angle, *d*. The central or diagonal arrow would be directed from the angle *a* to the angle *c*; and, in the same way, the system of arrows may be considered to be situated with respect to any other heated angle. The system of arrows in Fig. 20 will also apply to either of the ends of the metal when uniformly heated between the angles.

113. As both faces of the zinc exhibited Thermo-Magnetism of the same character in all the preceding experiments, whatever has been stated concerning those experiments will equally apply to both sides or flat faces of the metal, and, I imagine, to all similar masses of zinc. I must here observe, that the electric forces very seldom reach to the cold end of the mass, but approximate thereto in proportion to the advances of heat. They are the most powerful near to the heated point, and become more and more languid as they recede from it, till at length their energies are entirely lost in the more remote parts of the metal.

114.—*Experiments with Masses of Copper.*—Copper is one of those metals, the thermo-magnetic energies of which are not very easily detected in separate individual masses, unless they be of large dimensions. The most satisfactory results I have ever obtained from experiments on this metal were elicited by a rectangular mass, about two inches thick, and weighing about ninety-five pounds. This large mass of copper, which, by the interest of Mr. Marsh, I was permitted to examine, belongs to the Royal Arsenal. The experiments were made in precisely the same manner as those described with masses of zinc; and the results, excepting in *degree*, were exactly the same in both metals. The thermo-magnetic energies were very promptly and uniformly displayed in this mass of copper, but were exceedingly feeble when compared with those developed by a mass of zinc less than half its size. With the latter metal, a needle, on which the terrestrial magnetic powers were in full play, could be made to sweep an arch of 100°; whilst, with the unwieldy mass of copper, it required the soliciting terrestrial force to be entirely cut off from the needle in order to obtain a sweep of 6° or 8°.

115. There does not seem to be such uniformity in the display of Thermo-Magnetism by thin metallic plates, as is observed to be developed by those of considerable thickness. The phenomena, when thin plates are employed, although the metal be neither rolled nor hammered, assume a very capricious character, and appear to be governed by laws which are not easily traced to any general standard.

116. I am not at present prepared to say to what cause these phenomena are attributable: they seem to be of a distinct order, and not referable to the laws of crystallization. They may possibly be traced to a difference in the progress of heat in the several parts of the metal, moving with different degrees of celerity in the margin and body, or area of the mass. Should this conjecture be correct (and I have some reasons to think that it is true), I imagine that this class of experiments will exhibit a very prominent feature amongst all those which, from time to time, have been advanced for the solution of the highly important problem of terrestrial Magnetism, more particularly in that branch of the inquiry which relates to the diurnal variation.

117.—*Experiments with Spheres of Zinc.*—To carry the analogy of experiment still closer to terrestrial magnetic action, I have had cast globes of zinc of different sizes,

with a view of detecting some law by which their thermo-magnetic energies are exhibited when heat is partially distributed over the surface of the sphere, in imitation of the sun's action on the face of the earth. One of these globes is solid, and about 5·54 inches diameter, weighing nearly 23 pounds; another, which is hollow, and 10 inches diameter, weighs 64 pounds, the thickness of the metal being about 0·75 inch.

118. With these spheres I have as yet gained but little information, owing, as I suppose, to the difficulty which I have experienced in keeping the various parts of their surfaces at temperatures sufficiently remote from each other. I have, however, succeeded in deflecting a needle, by applying to it the ten-inch globe, partially heated, to a much greater angle than any that has ever been ascribed to diurnal variation. This result, insignificant as it may appear, and far from answering my expectations as to the extent of magnetic power developed by the sphere, sufficiently warrants the prosecution of the inquiry. The experiment has demonstrated the existence of the magnetic power in a homogeneous sphere of zinc, and the development of that power by heat. The field of inquiry is thus successfully penetrated, and future investigations may possibly lead to most interesting results.

119. A sphere of ten inches diameter, which is the largest I can at present command, is much too small for experiments of this character. With a globe thirty or forty inches in diameter, experiments might be made on a magnificent scale, and, I apprehend, with the most satisfactory results. A metallic sphere of such dimensions, with the necessary machinery for experiment, would require a sum which, perhaps, but few individuals would be found willing to lay out on an inquiry which is more of a national than of an individual interest. Researches of this nature would be the most likely to be successful, were they pursued under the patronage of governments, or of wealthy scientific associations. The experiments might then be carried on under advantages the most favourable to insure regularity and uniformity in the results, provided they were conducted under the superintendence of persons who have proved themselves competent to the task. They might also be pursued to an extent which no individual could hope to arrive at, and with a success that probably might at once set this sublime philosophical problem completely at rest.

N.B. I have succeeded in magnetizing an iron sphere by means of a thermo-electric combination. The same sphere becomes very highly magnetic when under the influence of the Electricity excited by a small Galvanic pair immersed in salt water; giving *direction* and *inclination* to a magnetic needle, highly imitative of those phenomena as exhibited by the action of the earth. A description of the apparatus, and the mode of experimenting, will be given in my next communication.

Artillery Place, Woolwich.

SUPPLEMENT TO THE SECOND MEMOIR.

ADDITIONAL EXPERIMENTS.

In the detail of my experiments on the Thermo-Magnetism of simple metals, published in the *Philosophical Magazine and Annals of Philosophy*, for January, 1831, I observed that I had some reason for supposing that, as the heat in a crystalline group meets with more obstruction in passing in one direction than another, this difference in its progress was probably the sole cause of the electric currents constantly observing one uniform direction with regard to the point of heat (see note to 42.) The experiments which led to this supposition, I remarked, could not be detailed with propriety in that place. The reason was, that as those metals (antimony and bismuth I was then speaking of), when pure, invariably exhibit *local* currents, which return into themselves in the same piece by various windings; and, as those currents always affect the *general* current in the circle, it was necessary to explain the operation of those *local* currents in the first place, and to guard against their influence when contemplating the operation of the *general* or principal currents, supposed to arise from other causes than that of crystalline groups of metallic films.

For this purpose I cast rectangular frames, of the same fashion as those I had before employed, of an alloy of tin and bismuth, in which no *local* or other current could be detected, to whatever point heat was applied, owing, no doubt, to the crystallization of the bismuth being nearly neutralized by the admixture of tin.

When one end of a frame was cut open by a fine saw, and one side of the opening warmed in the flame of a spirit lamp, the whole frame became magnetic whenever the warm and cool sides were brought into contact, as if an electric current set through the saw-cut *from* the heated *to* the cool extremities. I employed no multiplying Galvanometer; the two sides of the opening were simply sprung together by pressure between the finger and thumb, and the operation on the needle carried on as with the whole frames of antimony and bismuth, &c.

I had placed a good deal of importance in this discovery, until I found that the phenomena were not uniform in all the metals; for, although the current passes through the opening *from* the heated to the cool extremity in some metals, as in copper, brass, &c. it as constantly flows in the opposite direction in zinc, iron, &c.— just as I have shown to be the case with brass and steel partially hardened, and perhaps for the same reason. The facts, however, are certainly interesting—in a theoretical point of view, none perhaps are more so. They tend to give countenance

to the idea, that the *general* currents as well as the *local* currents, in the experiments with pure bismuth, antimony, &c. are generated in the crystalline groups of metallic films.—*Philosophical Magazine, for November*, 1833.

Note.—Although the original publication of this Memoir did not take place till July, 1831, it was written in its present form in the autumn of 1830; and, in consequence of suggestions by a member of the Committee who managed the *Journal of the Royal Institution*, it wa splaced in the hands of that Committee for the purpose of its appearing in their Journal for February, 1831; but in this I was disappointed. The Journal being a quarterly, the next opportunity for the appearance of my paper could not happen till May, and it was again submitted to the consideration of the Committee. But it was too severe an ordeal for my humble paper to pass through with credit; for, although "the Committee liked the subject," there were certain *terms* or *titles* employed which they did not understand—and another fault was, I had given *no reason for the facts!* Such were the circumstances attending the probation of this unfortunate Memoir; and, although Dr. Faraday (who was one of the Committee, and my only correspondent) very politely favoured me with suggestions for its revision, I saw no means of altering it to advantage; and, not being inclined to mutilate either the facts or their arrangement, I requested its return, and it arrived April 29th, after a probation of about four months. As I could not get it published in the *Philosophical Magazine* till July, a delay of about half a year was thus occasioned.

I have a particular interest in the history of this series of researches, whether individually considered or in connection with others, which are alluded to in a note appended to the "*Third Class of Experiments*," page 70, whose appearance was postponed in consequence of the above-mentioned delay in the publication of the present series. It is obvious, by the note at page 70, that I was led to expect some peculiar results by magnetizing the bismuth castings. The facts alluded to were some of those described in the ninth and tenth Memoirs—which show that I was not only in close pursuit of Magnetic Electricity, but had absolutely developed its *magnetic* phenomena, as well as the laws of their action on a compass needle, as displayed on excited masses or plates of copper, more than twelve months previously to Dr. Faraday's discovery of the spark, by magnetic excitement, became known. It appears, however, that we were both in quest of the same object at the same time; and, although we pursued different routes or modes of experimenting, our progress was nearly the same at the time Dr. Faraday's *First Series of Researches* was made known to the Royal Society.

EXPERIMENTAL AND THEORETICAL RESEARCHES IN ELECTRO-MAGNETISM.

THIRD MEMOIR.

The History of Electro-Magnetism has hitherto furnished no instance of its rotatory motions being accomplished, by the employment of both poles of a straight bar magnet, at one and the same time. The whole of the phenomena of that class have invariably been effected by the reciprocal action of one of its poles and a portion of the conducting circuit, under various forms, which joins the copper and zinc sides of the battery; and when either the position of the poles of the magnet, or the direction of the electric current is reversed, the rotatory movements become reversed also; but if both the current and the magnet are inverted at the same time, the direction of motion undergoes no change.

From these facts, in connection with the appearance of a neutral plane at the centre of the magnet, and at right angles to its axis, in which an electric current seeks to repose, it is possible that philosophers may have entertained the idea of the impracticability of obtaining rotatory motions by bringing into action both poles of the magnet at the same time; but, whatever may have been the cause, it is certain that no attempt of the kind has hitherto been recorded in the Journals of Science, with the exception of that noticed in the postcript to my first Memoir, dated April 13th, 1824.

The experiment alluded to, although suggested whilst reading Dr. Halley's famous hypothesis of terrestrial Magnetism, has no pretensions whatever to support the theoretical views of that eminent philosopher; nor is it intended to infer from the results of this experiment, whatever analogies may appear in them, that the rotatory motions of the earth and planets are due to electro-magnetic forces. The principal object of this part of the Memoir is to describe the character of an experiment which seems to introduce a novel feature to electro-magnetic movements, and to develop an association of polar action in magnets, on electric currents, not previously noticed.

The structure of the apparatus by which the experiment is made precludes the possibility of any other movement than that of its Voltaic parts, which perform a rotation around a central system of magnets; but there appears a high degree of probability that, were the magnets unconstrained and qualified with freedom of motion, they also would rotate at the same time—*i. e.* both the Voltaic and magnetic parts of the apparatus would rotate together.*

* I am not aware that any experiment, in which both magnet and battery have rotated together, has yet been recorded in the catalogue of electro-magnetic phenomena, although upwards of twenty-three years have elapsed since this idea was first made public; but an experiment which has long been made, and will be described in the present Memoir, will show the correctness of this view.—W. S. Kirby Lonsdale, Dec. 13, 1847.

Description of the Apparatus.—Fig. 1, Plate III. represents an elevation of the apparatus, in the axis of which is a brass cylindrical tube or case, N S, containing seven bar magnets, each of which is eight inches long. The magnets are placed in a frame within this cylinder, which keeps them firm in their places, parallel to its axis and to one another. The upper end of the cylinder is furnished with a moveable lid or cover, which gives facility for changing the positions of the magnets, when required, for varying the experiment. Fig. 2, Plate III. is a transverse section of the brass cylinder, with its contained magnets.

As the magnets are all placed with their *similar* poles in one direction, having their north poles at one end of the case and their south poles at the other, they act in concert as a compound magnet. At the lower end of the cylindrical case is a stout brass stud, or short vertical pillar, which stands firmly in a socket at the top of the mahogany foot. Around this socket is a cylindrical cell, *c c*, for the purpose of holding quicksilver. Another annular cell, *o o*, fits gently on the outside of the cylindrical case, about half way between its extremities, N and S: it is furnished with a metallic strap, the two ends of which are soldered, opposite each other, to the upper part of the inner rim of the cell. By means of this strap, which passes over the superior extremity, N, of the magnetic case, and reaches to the mercury in the cell, *o o*, the latter is sustained in its place. By means of two other narrow metallic straps, one on each side of the case, the quicksilver in the cell, *o o*, is connected with that in the lower cell, *c c*, at the foot of the apparatus.

The Galvanic part of this apparatus consists of an annular cylindrical copper vessel, *b b*, about seven inches diameter, in which is placed a rim, or loop-like strip, of thin rolled zinc. This arrangement will be better understood by Fig. 3, which is a transverse section of the whole sphere at its equator. The annular copper vessel is about three-quarters of an inch wide between its inner and outer rims, *c* and *r*, and the edge of the zinc cylinder is represented between these two rims. One end of each of two copper wires, *z z*, is soldered to the upper edge of the zinc, at about 180° from each other: their other ends are finely pointed, and dip into the quicksilver in the central cell, *o o*, as shown in Figs. 1 and 3.

To the outer rim of the copper vessel, Fig. 1, and at 90° from each other, are soldered four brass studs—to the extremities of which are soldered two brass wire circles (one only is shown in the figure), that cross each other at right angles in the pole, P. From P descends a pivot, that runs in a hole made to receive it in the upper part of the strap at N. The two circles are each nine-and-a-half inches diameter, and form two great circles of the sphere they are intended to represent, and of which they form a part. The lower parts of the circular wires are left open, and the points of them bent downwards till they come into contact with the quicksilver in the lower cell, *c c*. At the equatorial part of the apparatus is fixed another great circle of the spheres, at

right angles to the two former: it is represented by the circle, E E E E, in Fig. 3. Things being thus arranged, and diluted nitric acid poured into the copper vessel, *b b*, electric currents immediately flow throughout every part of the meridional brass circles—*ascending* in the upper hemisphere, from the equator, E E, to the pole, P; and *descending* in the lower hemisphere from the equator to the quicksilver in the lower cell, *c c*, which is the situation of the other pole of the sphere. From the poles the currents are transmitted to the quicksilver in the central cell, *o o*, by means of the metallic conductors, or straps; and both circuits are completed by the copper wires that reach from that portion of quicksilver to the zinc cylinder of the Galvanic pair. The arrows in Fig. 1 indicate the directions in which the electric currents flow in every part of the apparatus.

By this arrangement, two systems of electric currents are formed from one individual source of action; and hence, also, is solved the *apparent* anomalous phenomena displayed by electro-magnetic forces when *similar* electric currents are acted on by *dissimilar* magnetic poles. In this case the polar forces act in concert, and the sphere rotates by their joint influence. The delusion arises from the relative positions of the apparatus and the spectator; for in reality, as this experiment amply demonstrates, the rotations of similar currents around the two magnetic poles are, with reference to the magnet itself, constantly performed in one uniform direction. This fact is sufficiently obvious whilst the apparatus is permitted to remain in one and the same position; but, if it were to be inverted, the very same motion would *appear* to be carried on in the opposite direction.

To show this latter fact, we have only to invert the poles of the magnets within the cylindrical brass case, which would amount to the same thing as if the whole apparatus were inverted, because *similar* electric currents would traverse the two hemispheres, from the equator to the poles, as decidedly in the one case as in the other; but the spectator now sees the sphere rotating in the opposite direction—a delusion which could not have occurred had he taken the precaution to stand upon his head, or by any other means inverted his own position, whilst viewing the rotation performed by the *inverted* apparatus. It is a similar case to that which would occur to a person changing his position from one side to the other of a grinding-stone in motion. On looking at the revolving stone, its upper edge would move towards his right hand whilst in one of the positions, but to his left hand whilst in the other.

Owing to the weight and size of the apparatus, its motions are but slow at the commencement of the experiment, but soon acquire a sufficient velocity to render them striking and impressive. The appearance of the apparatus is much improved by conferring upon it something more of the character of a sphere, which is accomplished by the addition of a few more meridional circles of brass wire, and polar circles of similar material. Fig. 4, Plate III. is a perspective view of the apparatus when

properly equipped with the additional appendages. In this form it has a very imposing effect on the lecture table, and its steady rotatory motion renders its appearance at once stately and majestic.

The success that attended this experiment in all its particulars, so fully satisfied my expectations, and realized the views I had taken respecting the *apparent* anomalism in the laws which govern electro-magnetic rotations, that there appeared every facility afforded for the application of the same principles to other forms of apparatus, and to the production of novel and interesting facts.

It is well known that, although attempts had previously been made, the rotation of a magnet on its axis, by the force of an electric current, was first accomplished by M. Ampere; but, as in all other cases of electro-magnetic rotation hitherto known, the phenomenon was exhibited by operating on the poles of the magnet separately, and thus limiting the action to one individual pole only in each experiment—which is a remarkable feature in the mode of experimental inquiry, up to this period, in the history of Electro-Magnetism. But, notwithstanding the conclusiveness of the experiment already described, and the effects it is calculated to produce in dispelling the illusion referred to, the illustration of a more extensive application of the same principle will be considerably amplified by an attentive examination of the distribution of the electric and magnetic forces in the following experiments.

The Rotation of a Magnet on its Axis by subjecting both of its Poles at the same time to the iufluence of similar Electric Currents.

Fig. 5, Plate III. is a vertical section of the apparatus, with the batteries *detached;* and Fig. 1, Plate IV. is a perspective view with the batteries *attached.* With the exception of the magnet, N S, Fig. 5, Plate III. the apparatus is of brass work. A is a round foot, from which rise two pillars, B B, for the purpose of supporting the annular cell, *e e*, holding quicksilver, and also the cross piece, *r*, which is screwed down securely to the shoulders of the pillars by the nuts, *k k*, and thus the frame of the apparatus is kept firmly together, and stands perfectly steady. The magnet passes through the opening in the centre of the cell, *e e*, and communicates with the quicksilver by means of a wire, *f*, soldered to its equator, and at right angles to its axis: the wire is bent and finely pointed, as in the figure, so as to dip into the quicksilver and move freely whilst it is immersed in it. The poles of the magnet are furnished with pivots, the lower one of which, at S, runs in a small cup on the top of the foot, A; and the upper one, at N, runs in the lower extremity of a screw which passes through the cross-piece, *r*. The head of this screw is formed into a cup, for the purpose of holding

a drop of quicksilver, and receiving one end of the conducting wire from the battery. C and Z represent two wires proceeding respectively from the copper and zinc sides of the battery ; the former communicating with the upper pole, N, as shown in the figure, and the latter communicating with the quicksilver in the cell, *e e*, and by means of the short wire, *f*, the connection is continued to the equator of the magnet. Thus the upper half of the magnet forms a portion of the electric circuit, through which the current flows *from* the equator to the pole. The other half of the magnet also becomes a channel for a similar electric current, which flows downwards from the equator to the pole, S. This current is supplied from another battery, which is connected with the apparatus by means of the wires, C and Z. (See also Fig. 1, Plate IV.)

By subjecting the magnet, in this manner, to the influence of two similar electric currents, it is rotated with an astonishing velocity, which may be abated in a moment by interupting the circuit of either battery, and again accelerated by renewing the connections. In consequence of the facility with which one, or both, of the currents can be applied to the magnet, this experiment is better calculated than the former for illustrating the concert of the forces of the two electric currents, when employed at the same time on both polar regions of the magnet, in the manner thus described; and how completely they neutralize each other's action when the electric currents flow in opposite directions with reference to the poles and equator of the magnet. This latter effect is manifested by a very slight change in the arrangement represented by Fig. 5, Plate III. ; for, if one of the currents be reversed whilst the other remains undisturbed, the rotation of the magnet is immediately arrested, by the counteracting forces which balance each other in its two halves.

Besides the novel views which this apparatus is calculated to disclose, it is susceptible of a variety of vicissitudes of adaptation of the electric and magnetic forces, which render it exceedingly convenient to the experimentalist, and qualify it for inspiring a renewed ardour for electro-magnetic inquiries. The comparatively large size of the magnet,* and its full exposure to view, afford ample accommodation and facility for observing the phenomena it is enforced to display, under all the variety of connections it is adapted to form, with the sources of electric action that may be employed—whether in its present position, or with its poles in the reverse order; whether its poles be operated on separately, or contemporaneously; whether by individual, or by confluent electric currents; and whether the currents employed act in concert, or be adverse to each other's action.

Another convenience has been accomplished in the performance of electro-magnetic experiments generally, by the diminutive size and compactness of the batteries that

* The magnet is eight inches long. That employed by M. Ampere was about the size of a small goose quill, and half of it concealed from view by immersion in the mercury, in which it floated, with its axis, in a vertical position.

I employ. They are of such a peculiar construction as not to annoy the experimenter by the escape of hydrogen, or other gas, whilst in operation; neither is the expense of making the experiments more than one-fortieth of that incurred by any other mode yet made public, although the apparatus operated on are of unusual large dimensions.

W. S.

Woolwich, August, 1824.

SUPPLEMENT TO THE THIRD MEMOIR.

EXPERIMENT SHOWING THAT AN ELECTRO-MAGNETIC SYSTEM, SUSPENDED IN SPACE, WILL ROTATE ON ITS AXIS.

I am not aware that the experiment now about to be described has ever before been published beyond the precincts of my own lecture-table, although it was first made in the year 1825. It was instituted for the purpose of amplifying the views which the two former experiments seemed to disclose, respecting the possibility of the earth and planets being kept in a state of rotatory motion by electro-magnetic forces; but how far it is calculated to affect the judgment of those philosophers who are most likely to form sound opinions on this topic is not for me to determine.

The apparatus consists of a magnetic bar of steel, N S, Fig. 6, Plate III. and an annular Voltaic battery, *b b*. The extremities of the magnet are drawn out and finely pointed, so that they may run freely in pivot holes in the top and bottom of the frame, *f f*, F F. The battery consists of a narrow annular cell of copper, *b b*, and a circular strip or hoop of thin rolled zinc, placed within it, as shown by the figure.

The copper cell is firmly attached to the centre of the magnet by means of four brass studs and solder, and the zinc is attached to the two brass-wire semicircles, *c c* and *c' c'*, which are soldered to the poles of the magnet. When diluted nitric acid is poured into the battery, the electric currents proceed from the copper cell, through the metallic studs, to the equator of the magnet; thence they proceed through the magnet, in both directions, to its poles. From the poles, the currents are transmitted along the metallic semicircles, *c c'* and *c c'*, to the zinc. The directions in which the currents flow through the principal parts of the arrangement are indicated by the arrows.

In consequence of the weight of this apparatus, its motions are but slow, when compared to the rapidity with which the magnet revolves on its axis (Fig. 5, Plate III.) when disencumbered of the Voltaic battery. The illustration intended, however, is complete and decisive.

Observations on the Analogies displayed by Electro-Magnetic and Astronomical Phonomena.

Whatever may be the views that philosophers have entertained respecting the nature of the forces that maintain an unremitting state of rotatory motion in each individual body constituting the solar system, the similitude observable in the celestial phenomena, and those developed by Electro-Magnetism, is sufficiently remarkable to engage the attention, and to lead the contemplative observer to an unavoidable cognition of analogies of an exceedingly interesting character. The rotations exhibited by the experiments detailed in this Memoir are highly imitative of those performed by the earth and planets, on their respective axes; and the arrangement and operation of the electro-magnetic forces, in the last described experiment, cannot be looked on with indifference by those who are in the habit of studying the rotatory motions displayed by the celestial bodies.

It is well known that conjecture is all that has hitherto been advanced to furnish philosophers with an idea respecting the primary cause of celestial motions. Nor, indeed, has conjecture itself conducted the mind beyond certain supposed operations in the mechanical processes themselves—certain imaginary impulses, suitably applied to the bodies and admirably adapted to mathematical rule, without assigning any cause for the impulses the hypothesis requires.

In no branch of science, however, has cautious speculation been more required than in that of celestial mechanics; for, notwithstanding the conspicuity of the phenomena, and the mathematical regularity of their display, philosophers were not favoured in their conjectures by experimental analogies until the recent discovery of Electro-Magnetism. No force but that of gravitation had been recognized amongst the planetary bodies, although other forces were required, and unscrupulously invented by mathematicians, to bring rotary motions to within the range of their art. Electro-Magnetism, however, seems to promise the commencement of a new era in speculations of this interesting nature, when the completeness of the analogies, and the simplicity and ampleness of their production, are taken into close consideration.

Independently of any suppositive data for the formation of an hypothesis, it is demonstrated by the last described experiment in this Memoir, that a magnetic body, carrying appropriate electric currents, is all that is necessary for the production of rotatory motion in the system of the bodies employed. And, with respect to the rotation of the earth, or any of the planetary bodies on their respective axes, it is only necessary to consider them as magnetic bodies, and susceptible of perviousness to electric currents of their own production.

That the earth is endowed with magnetic powers is a well known fact, and that it is an electric conductor is equally certain; and, by considering that our miniature

batteries are constructed of mineral substances, extracted from the bowels of the earth, which abounds with them in almost every form of association, the required elements for the production of the phenomena would appear to exist in the constitution of the earth itself; and, by analogy, the same reasoning applies to every primary planet in the system.

But it is now well known that Electricity can be excited by heat, and the success of thermo-magnetic rotations, when the apparatus is appropriately formed into the the shape of a sphere, may be made to depend upon the difference of temperature between the equator and the poles. This condition of the apparatus is so obviously analagous to the natural state of the earth, that one might be led to imagine a continuous generation of electric floods, by the action of the sun between the tropics, which would sweep the globe thence to the poles, and return within the interior in an endless circuit of conductors.

Another circumstance favourable to the supposition that planetary rotations are of electro-magnetic production is, that a magnetic body (a sphere, for instance), suspended in space, and having electric currents traversing its substance, either wholly or partially, from its equator to its poles, would not only rotate on its axis, but also maintain its parallelism, provided no other forces were in operation to disturb the electro-magnetic effects; for, although a magnetic body, under certain circumstances, is subject to deflections from the original position of its axis by *external* electric currents, it has no tendency to change its position, by similar currents, when transmitted through its own substance in those directions which force it into rotation on its axis.

When these facts, most of which are deducible from experiment, are taken into consideration, there can appear no extreme improbability in supposing that most of the phenomena which are observed to obtain amongst the bodies composing the solar system, are the offspring of Electro-Magnetism. Rotation and parallelism, we have seen, are amongst electro-magnetic productions; and the action of the sun may possibly be sufficiently influential to produce the requisite electric currents. Under this supposition, magnetic bodies, such as the earth, suspended in space, and exposed to the heating influence of the sun, would display rotation and parallelism; and it is possible that other interesting analogies may be discovered amongst electro-magnetic phenomena. W. S.

Woolwich, *August*, 1824.

Second Supplementary Experiment.

The experiment I am now about to describe, which has been copied into some popular works on natural philosophy, was first shown in a lecture which I delivered at Woolwich, in the winter of 1824. It was instituted with a view of demonstrating

some theoretical notions which I then entertained, and which will be particularized in the seventeenth Memoir. It is described in this place because of its being of the same class as the three preceding experiments in this Memoir.

The apparatus is represented by Fig. 7, Plate 3. It consists of two bar magnets, N S and N S, fixed on the opposite sides of a stout brass wire, Z Z, but kept from contact with that wire by two square pieces of cork. From the centre of the wire, Z Z, proceeds a short copper wire, which is bent downwards at its outermost extremity at C, and is finely pointed. Similar poles of the magnets are placed in the same direction, and held firmly in their places by binding wire.

When this apparatus is placed in a frame similar to that in which the single magnet is situated in Fig. 5, the point of the wire, C, dips into the mercury in the annular cell, *e e*, and is thus brought into contact with the copper side of the battery; and the two extremities of the axial wire, Z Z, are connected with the zinc side. By these means the current flows through the axial wire in both directions, from its centre to its extremities; and the whole system of magnets and wire rotate with an astonishing velocity. W. S.

RESEARCHES IN ELECTRO-MAGNETISM.

IMPROVED ELECTRO-MAGNETIC APPARATUS.

The Large Silver Medal and Thirty Guiueas were this session presented to Mr. W. Sturgeon, 8, *Artillery Place, Woolwich, for his Improved Electro-Magnetic Apparatus. A set of which has been placed in the Society's Repository.**

Mr. Marsh's apparatus, for the same purpose as Mr. Sturgeon's, was rewarded by the society in the session before last. The battery, consisting of plates presenting eight surfaces of about a square foot each, and weighing from twelve to thirteen pounds, was the smallest that had at that time been applied to electro-magnetic researches. The rest of his apparatus, with few exceptions, was such as had already been used by Ampere, Sir H. Davy, Mr. Barlow, Mr. Faraday, and others, fitted together in a very covenient way, and stowed in a box, which, with the battery, made a package sufficiently portable.

Mr. Sturgeon's apparatus is even more portable than Mr. Marsh's, and (the moving parts being at the same time larger) is better fitted than that for the use of the lecturer.

* Extracted from the Transactions of the Society of Arts, for 1825.

The battery is similar in construction to Professor Hare's calorimotor, and consists of two fixed, hollow, concentric cylinders of thin copper, having a moveable cylinder of zinc placed between them. Its superficial area is only 130 square inches, and it weighs no more than 1lb. 5oz. It is moveable on an upright metallic rod, like a laboratory lamp, and may, therefore, be adjusted to any height. It will not hold more than about a pint of liquid, which should be composed of one part nitric acid and eleven parts of water: it is charged with the greatest ease, by merely pouring in the liquor from a common lipped jug. A further advantage of the construction is, that after every experiment the liquor may be returned to the jug, while preparations are making for the next, by which the battery, when wanted, is in a high state of activity, and is undergoing no deterioration in the interval between one experiment and another.

A further point of novelty is, that Mr. Sturgeon has very judiciously chosen to have a small Galvanic power, assisted by a strong magnetic power, rather than the reverse, as is usually the case. This has enabled him greatly to economise in the size of his battery, and in the cost of acid to excite it, while the increased magnetic power is obtained at a small first expense, and needs no renewing.

Letter to the Secretary of the Society of Arts.

8, Artillery Place, Woolwich.

Sir,—The science of Electro-Magnetism, although so generally interesting, yet (comparatively speaking) appears to be very little understood. This latter circumstance is probably, in a great measure, owing to the difficulty of making the experiments, and the great expense attending the process; for, besides the first price of a large battery, considerable expense in acid must always attend its excitation, whenever an experiment is attempted. Large batteries are always attended with difficulty of management, and the great quantity of hydrogen evolved during the process renders the use of them extremely inconvenient to the operator. These are, evidently, great obstacles to the experiments being often repeated, and to the science being generally known. Another, and perhaps no less, obstacle to the advancement of this interesting science, is, that the experiments being hitherto exhibited on so small a scale, are by no means calculated to illustrate the subject in public lectures; for when the experimenter succeeds even to his wishes (which is not frequently the case), the experiment can only be seen by a very near observer, and the more distant part of the auditory are obliged to take for granted what they hear reported (from those persons who are more favourably situated) of some of the most interesting facts, which they, from their distance, are unable to witness.

With a view of removing, in some measure, these apparently formidable obstacles in the progress of this infant science, I have devoted a considerable portion of time, labour, and expense, in repeating several of the experiments, under various circumstances, and with various forms and sizes of batteries. I have likewise instituted a series of experiments, for the purpose of discovering, if possible, if any particular ratio of Galvanic and magnetic power was absolutely necessary to be observed in the process of Electro-Magnetism. If no particular proportion of those two powers was essential, then it appeared highly probable that an increase of magnetic power might compensate for a deficiency of the Galvanic, and thereby render the use of large Galvanic batteries quite unnecessary—an object which I considered both interesting in its nature, and by reducing the expense, and facilitating the process, exceedingly desirable to the experimenter; and I am happy to state, that my labours were no ways abortive, for instead of electro-magnetic phenomena depending on powerful Galvanic, and feeble magnetic force, as had till then been practised, I found, during that inquiry, that the Galvanic force may be reduced to almost any degree, provided the magnetic be sufficiently powerful. This discovery led me to the use of powerful magnets and small Galvanic batteries, for with small magnets the experiments can never be made on a large scale, although the Galvanic force be ever so powerful; and, as minute and delicate experiments are not calculated for sufficiently conspicuous illustration in public lectures, I considered that an apparatus for exhibiting the experiments on a large scale, and with easy management, would not only be well adapted to the lecture room, but absolutely valuable to the advancement of the science. Upon this principle I constructed a complete set of instruments, which, from their superior magnitude and peculiar arrangement, in my humble opinion, and by the certificates I have been honoured with are, in the opinion of gentlemen whose judgment I presume will ever be held in the highest estimation, well adapted for the illustration of the subject, either in the private study or public lecture room.

It will be understood from what I have already stated, as well as from an inspection of the instruments, that the mode which I have taken for the production of electro-magnetic phenomena is more simple in its management, less expensive in the process, better calculated for the illustration of the subject, and the reverse of that which has hitherto been used, and which, by its almost entire dependence on the tedious and expensive process of Galvanism, has considerably retarded and obscured this new and interesting science; for whenever an experiment was not attended with the anticipated success, the failure was generally attributed to an insufficiency of Galvanic power; and, in order to increase the effect, it appears that the experimenter had no other means of accomplishing his object, than by augmenting the power of his battery, or by reducing the size and increasing the delicacy of his other apparatus, the magnetic power being either entirely lost sight of, or regardlessly neglected, as if no ways materially concerned in the process.

I have found, however, by the above-mentioned course of experiments, that the magnetic force is as essential as that of Galvanism to the development of electro-magnetic phenomena; and the apparatus which I now submit to the attention and impartial consideration of your valuable society, acting on the principle of powerful Magnetism and feeble Galvanism, will, I trust, be found more eligible and efficient than any other that has yet been brought before the public.

I am, Sir, &c. &c.

A. Akin, Esq. Secretary, &c. W. Sturgeon.

CERTIFICATES.

Royal Military Academy, Woolwich Common,
9th May, 1825.

Sir,—As I understand that Mr. William Sturgeon proposes to submit his Galvanic battery and electro-magnetic apparatus to the consideration of the Society for the Encouragement of Arts, &c. I take the liberty of recommending them to your favourable consideration, although I have not the honour of being personally known to you. Since the first discovery of the action of the Galvanic circuit on the magnetic needle, I have witnessed all the principal phenomena connected with Electro-Magnetism, as exhibited by batteries and apparatuses of different constructions, and have no hesitation in saying, that the battery and apparatus of Mr. Sturgeon are most admirably calculated for exhibiting all these phenomena in the most striking manner, and that they afford peculiar facilities for investigation in this most interesting science. The expense attending the purchase and use of large Galvanic batteries, the difficulty of keeping them in powerful action, and the inconvenience of using them, except in laboratories, must all tend to retard the progress of a science which has opened a new field of inquiry; and as the battery and apparatus of Mr. Sturgeon are free from all these objections, I consider that, by rendering them public, the cause of science is promoted, and that he is deserving of every encouragement for their invention and successful application.

I am, Sir, &c. &c.

A. Akin, Esq. Secretary, &c. S. H. Christie, M.A.

Royal Military Academy, Woolwich,
May 9th, 1825.

Sir,—I beg leave to introduce to the favourable notice of the society, Mr. William Sturgeon, a resident of this town, who has for some time devoted himself most sedulously to the improvement of apparatus for the development and exhibition of the

properties of Electro-Magnetism. He wishes to solicit the patronage of your valuable institution to a new *battery* of his invention, which, while it is very far smaller than any other of which I have heard, is very efficacious, and at the same time remarkably calculated to facilitate the experiments. Mr. Sturgeon has also, in my judgment, greatly improved several other classes of apparatus employed in this interesting department of research, and constructed them upon a scale which renders the experiments susceptible of pleasing and successful exhibition to a large auditory. He will regard himself as highly honoured by permission to exhibit them before a committee of the society; and I cannot but believe, that if he be so honoured, his talents and ingenuity, tending as they have done to most curious and pleasing results, will entitle him to favourable consideration and an appropriate reward.

I am, Sir, &c. &c.

OLINTHUS GREGORY,

A. Akin, Esq. Secretary, &c. *Professor of Mathematics.*

Having read the above certificate of Dr. Gregory, and that of Mr. Christie, I have no hesitation in adding my testimonial to every thing there stated.

PETER BARLOW.

Reference to the Engravings of the Electro-Magnetic Apparatus.—Plates IV. and V.

Plate IV. Fig. 1, a perspective view of an apparatus to show the revolution of a magnet round its own axis. *a a*, the two Galvanic apparatuses on their stands, *b b:* they are acting on the magnet, N S, by means of the connecting wires, *d d d d;* both their copper poles, C C, are applied to the equator, *e*, of the magnet; while the zinc pole, Z, of one is applied to the north pole, N, and the zinc pole, Z, of the other is applied to the south pole, S, of the magnet. A wire, *f*, is soldered on to the magnet, and bent down at one end to dip into the circular trough, *e*, to form the equatorial connection; and as all the connections are made by mercury and amalgamated wires, the end of this wire is amalgamated, and mercury put into the trough: all the little cups, Z and C, are also amalgamated at the bottom, and contain mercury; the bottom wires of the zinc and copper poles are likewise amalgamated to dip in connecting cups when wanted. The magnet has brass wire centres on which it turns; that at the north pole stands in a cup at Z, with mercury; and the other at the south pole enters the amalgamated hollow in the screwed end of the upper connecting cup, Z. When the connections are made, as above described, on pouring in dilute nitric acid into the troughs, *a a*, the magnet will revolve in the way shown by the arrow; but

on changing the connections, by applying the copper wires to the poles and the zinc ones to the equator, it will revolve the contrary way: here the magnet only forms the connection between the electric poles, and revolves around, or with the current which is conducted by it. *g g g*, is the stand which supports the magnet; the equatorial trough, *e*, is made moveable on the pillars, *g g*, and fixed by the screws, *h h*.

Fig. 2, a bird's-eye view of the same, without the stand, *b b* and *g*. The Galvanic troughs, *a a*, are copper and cylindrical, having a smaller split cylinder of copper soldered within to increase the copper surface: the intermediate cylinder in each trough is zinc; it has three cork feet, cemented to the bottom edge to prevent contact with the copper, and two pieces of cork, *i i*, cemented at the top for the same purpose: *e* the annular trough.

Fig. 3 shows the magnet separate.

Fig. 4, a wooden cover; one is fitted to each trough, *a a*, to prevent the ebullition of the acid from damaging the apparatus.

Figs. 5 and 6, a side and front view of a circular metal disk, made to revolve between the poles of a horse-shoe magnet; the disk is amalgamated round its edge, and dips into a little mercury contained in a hollow, *j*, of the stand; the centres, *k k*, on which it turns, and the hollows that receive them in the forked support, *l l*, are amalgamated. The screw, *m*, allows the disk to be adjusted, and fixed so as only just to touch the surface of the mercury. A horse-shoe magnet, N or N S, shown by dotted lines, is laid on the stand, then one of the troughs, *a*, of Fig. 1, is to be adjusted on its stand, *b*, till its bottom wire, Z, dips into the connecting cup, Z, forming the zinc communication, and a connecting wire, *d*, with bent ends, is to dip into the copper connecting cup, C, of the trough, and into the cup, *c*, of the disk. The communication of the poles being thus made, (the current passes from Z through the mercury, *j*, into the edge of the disk, and through its centres, *k k*, into the fork, *l l*, and up to the cup, *c*), the disk will then revolve as shown by the arrow. By reversing either the poles of the magnet, or the electric poles, the revolution of the wheel is reversed; but if both are reversed, the revolution will continue in the same way as at first. The six rays are painted on the disk, merely to render the revolution visible at a greater distance.

Fig. 7, a stand supporting a needle between two conducting wires, *o o* and *p p*, to show the different effect of Electricity on the needle when passing above or below it; the cup, Z, is common to both, but the other ends have each a separate cup, C C. When the electric current passes along the upper wire, *p p*, the needle takes the position shown in Fig. 8; but on lifting the connecting wire out of the cup, *p* C, and putting it into the cup, *o* C, the current passes under through the wire, *o o*, and the needle immediately goes round to the position indicated in Fig. 9: then if you watch the motion of the needle, and keep alternately transferring the wire out of one cup

into the other, keeping time with the needle, you may bring it into the most rapid revolution that you can possibly keep time with.

Plate IV. Figs. 19 and 20, show a side and front view of a dipping needle, mounted between two wires, *o* and *p* ; they are here placed in the direction of the dip, but the quadrant, *i*, allows them to move one quarter round, or to the equator of the magnet, as shown by dotted lines. In their present position the needle will deviate, as Figs. 8 and 9, Plate IV; and it will be seen the needle cannot take a position quite at right angles to the wire, owing to the terrestrial Magnetism drawing it on one side; but when the wires are carried round to the dotted position, Fig. 19, Plate IV. the needle remaining as it was, so as to be at right angles to each other, then on passing the current from z through the wire *o o*, no effect will appear to take place—the needle is only more confirmed to its position; but on passing it through *p p*, the needle goes round, and dips with its south pole. The wire passes through the wooden cup, z, but the two ends of it, *p* and *o*, only just enter their respective wooden cups, c c; these wooden cups are placed at an angle of 45° to the horizon, so that in either position they are similar, and will hold mercury enough to make the contact.

Plate IV. Fig. 10, shows an arrangement for making a cylinder revolve by applying a battery of magnets to the outside. The cylinder and stand are in section, to show the support, but are too far separated from the magnets in the engraving.

Fig. 11 is a bird's-eye view of the same apparatus, the cylinder being removed, and its place shown by a dotted circle; the middle portion, *q*, contains mercury, which also flows in a very narrow arm to the side where the magnets are placed. The sides of this narrow arm are so low that the convex surface of the mercury rises above them, and the edge of the cylinder, which is amalgamated, passes over, as shown by the dotted circle, and keeps in contact with the convex surface of the mercury without touching the sides of this little trough. The cylinder, Fig. 10, has a sharp point in its crown, by which it hangs freely on the top of the central wire, *r* ; this wire fits into a pipe or hole, *s*, in the stand, and the mercurial trough, *q*, being varnished, prevents this axis from forming a communication. There is also a little connecting cup, *t*, for mercury on the top of the cylinder, into which the screw of the upper connecting cup, c, dips just enough to touch the mercury; *v v* is a wooden stand holding six magnets, the north poles of which are placed near the cylinder on the side that dips into the mercury; then, on making the Galvanic communication with z and c, the current passes through the wire of z into the mercury, *q*, and out of the mercury up that side of the cylinder opposed to the magnets, and meets the copper pole, c, at top. Now the magnets are continually propelling that portion of the cylinder which forms the connection, and as the succeeding portions continue to form the connection with the mercury only opposite to where the magnets are placed, they are as continually

propelled, and thus a revolution is produced: the arrows show the direction of the motion, and changing the poles changes the direction.

Fig. 12, *w w*, shows the arm which supports the upper conducting cup; it fits into the pipe, *x*, which is screwed to the foot.

Figs. 13 and 14, a front and side view of a stand with two connecting cups, Z and C, made of wood, in which the bent iron rod, wound round with copper wire, is supported by the two copper wire ends. On making the Galvanic connection through the copper wire, the iron rod becomes a strong horse-shoe magnet, and will support a heavy bar of iron as *y*, Fig. 14; but on lifting the connecting wire, *d*, Fig 13, out of the cup, Z, the weight immediately drops, and, on restoring the connection, the power is restored; then if you change Z for C, it will change N for S, or if you only wrap the copper wire about the iron rod as a right threaded screw instead of a left one, as in the drawing, it will change N for S. This is explained by what takes place in Figs. 7, 8, and 9.

Plate V. Fig. 1, another copper wire fitting the stand. This arrangement communicates Magnetism to hardened steel bars, as soon as they are put in, and renders soft iron within it magnetic during the time of action; it only differs from Fig. 13, Plate IV. in being straight, and thereby allows the steel or iron bars to slide in or out.

Plate V. Fig. 2, shows the revolution of two magnets round two similar electrified wires; *g g*, a stand supported by three pillars, *h h h ; e e*, two annular wooden troughs, (one is shown separately in section, Fig. 3); the neck, *i*, fits easily into corresponding holes in the stand, *g g ; j j*, two standards with pointed tops; N S, S N, the two magnets are bent as Fig. 4, and have hollows in the middle, by which they swing on the standard points, *j j :* fine copper wires, *k k*, are twisted tight on each magnet, and go loosely round the standards, *j j ;* they serve to keep the magnets upright; *l*, a brass standard fixed at the back of the stand, and bent forwards at the top, where it is split to receive the flat piece, *m*, which is secured by the screw, *o ;* this flat piece carries the bent wire, *p p*, having cups, C C, on its shoulders, and points at the bottom, to enter without touching the cups, *q q ; f f*, Figs. 4 and 5, are bent wires, cemented together on the middle of the magnets; these pass through the cups, *q q*, and dip into the troughs, *e e*, Fig. 2; into these as well as into the cups, C C and Z Z, mercury is placed to form the connection, then bringing the Galvanic troughs, *a a*, and dipping the zinc poles into Z Z, at the equator of each magnet, and uniting the copper poles, *c c*, by the wires, *d d*, to the upper cups, C C, the circuits are completed, and the magnets will revolve, as shown by the arrows, around the wires, *p p*.

Fig. 6, a horse-shoe magnet, mounted with two mercurial troughs, *r r* (Fig 7 shows one separate); *t t*, two cylinders suspended on the ends of the magnets, by points within their crowns, under the cups, *v v ;* their bottom edges are filed away, leaving

only four points (as Fig. 8) to touch the mercury, by which means the friction is much lessened. The troughs are adjusted by the screws, *u u*, so as to bring the mercury just in contact with the points of the cylinder; the screw points of the upper cups, c c, just touch the mercury in the cups, *v v*. Upon making the communications, as before, with the cups, z z and c c, the cylinder will revolve, as shown by the arrows.

Fig. 8, *w* is one of the wires which holds the upper cups, c c; the bent end, *w*, twists into a hole in the side of the trough, *r*, and thus supports the wire when it is necessary to turn it over on one side previous to removing the cylinder.

Fig. 9 shows a stand on which a rectangular wire is suspended on one of its sharp pointed and amalgamated ends, which is here the copper end; the other, or zinc end, although tied to it, is kept separate by the silk thread passing between them; this zinc end dips into the mercurial cup, *x*, from which the small wire, *z*, descends to the cup, z, at bottom; the cup, c, is united to the standard (the cup, *x*, and the part of the standard that rises through it is well varnished, to insulate the mercury from the standard). On making the zinc and copper communication with these cups, the circuit is completed through the rectangular wire, and it will then take the position east and west; but, in changing the communication, the west side will go round to the east.

Fig. 10 shows another rectangular wire, through which the circuit is completed; the side, *y*, when offered to the west side of Fig. 9, repels it, the current being in contrary directions through them, as shown by the arrows, but it will attract the east side, the two currents being then alike.

Fig. 11 is a compound or double horse-shoe magnet, fixed upright in a block of wood, *a a*. Two brass tubes, shown in section, *b b*, are fixed in the board, *c c*, and fit on over the magnet, making them appear as two pillars: they are secured to the bottom block, *a*, by two screws, *d*. On the top of these are fitted the two circular mercurial troughs, *e e*—(they are shown separate in Figs. 12 and 13). The upper cups, c c, have loops, or eyes, at the bottom of their screws, which are amalgamated as well as the hooks of the wires on which the pith balls, *f f*, are placed. The lower ends of the wires dip into the mercury in the troughs, *e e*: on making the communication or circuit through z c, z c, the wires will revolve as shown by the arrows. The upper cup, c, is insulated from the arm, *w*, Fig. 13, by the piece, *u*, being of hard wood.

Fig. 14 shows one of a pair of troughs fitted to the same magnet; it has a fixed arm or arch, *g*, through the top of which passes a cup-headed screw, also a smaller arm with the stud, *h*, to hang on the top of the pillar, *b*, Fig. 11. The trough is varnished, to enable it to hold dilute acid. On the stud, *h*, hangs the zinc cylinder, shown separate, Fig. 15. It is adjusted by bringing the screw in the arch, *g*, to

touch the mercury in the little cup, then the copper cylinder, Fig. 16, is hung on the screw-head by its amalgamated point, completing the circuit through the screw of the fixed arch, *g* ; on filling these vessels with dilute acid, the zinc and copper cylinders will revolve, as shown in Fig. 17.

Fig. 18 shows two of the connecting wires separate; three or four pairs of each of these are required.

These figures are nearly one-fifth of the real size; and it will be seen that the magnetic power is very great in proportion to the Galvanic power.

ON THE PROPERTIES OF ELECTRO-MAGNETS.

FOURTH MEMOIR.

When first I showed that the magnetic energies of a Galvanic conducting wire are more conspicuously exhibited by exercising them on *soft iron* than on *hard steel*, my experiments were limited to small masses—generally to a few inches of rod iron about half-an-inch in diameter. Some of those pieces were employed while straight, and others were bent into the form of a horse-shoe magnet, each piece being encompassed by a spiral conductor of copper wire. The magnetic energies developed by these simple arrangements are of a very distinguished and exalted character, as is conspicuously manifested by the suspension of a considerable weight at the poles during the period of excitation by the electric influence.

An unparalleled transiliency of magnetic action is also displayed in soft iron, by an instantaneous transition from a state of total inactivity to that of vigorous polarity, and also by a simultaneous reciprocity of polarity in the extremities of the bar—versatilities in this branch of physics for the display of which soft iron is pre-eminently qualified, and which, by the agency of Electricity, become demonstrable with the celerity of thought, and illustrated by experiments the most splendid in magnetics.

It is, moreover, abundantly manifested by ample experiments, that Galvanic Electricity exercises a superlative degree of excitation on the latent Magnetism of soft iron, and calls forth its recondite powers with astonishing promptitude, to an intensity of action far surpassing anything which can be accomplished by any known application of the most vigorous permanent magnet, or by any other mode of experimenting hitherto discovered. It has been observed, however, by experimenting on different pieces, selected from various sources, that, notwithstanding the greatest care be observed in preparing them of a uniform figure and dimensions, there appears a

considerable difference in the susceptibility which they individually possess of developing the magnetic powers, much of which depends upon the manner of treatment at the forge, as well as upon the natural character of the iron itself.*

The superlative intensity of electro-magnets, and the facility and promptitude with which their energies can be brought into play, are qualifications admirably adapted for their introduction into a variety of arrangements in which powerful magnets so essentially operate, and perform a distinguished part in the production of electro-magnetic rotations; whilst the versatilities of polarity of which they are susceptible, are eminently calculated to give a pleasing diversity in the exhibition of that highly interesting class of phenomena, and lead to the production of others inimitable by any other means.

An experiment of this character is noticed in the *Philosophical Magazine*, for January, 1825;† but as the arrangement by which it is accomplished has not yet been published, a description of it in this place may perhaps still be interesting, especially as it affords a clue to several others which may be exhibited in this curious branch of science.

Experiment.—Fig. 1, Plate VI. is a representation of the apparatus complete. It consists of a cylindrical rod of soft iron, *i i*, supported in a vertical position by a round wooden foot, into which its inferior extremity is inserted. The superior extremity of the iron rod passes through the centre of a shallow wooden dish, *d d*, which is kept firmly in its place by means of cement. The inside of this dish and the iron rod are covered with sealing-wax varnish. A copper wire, one end of which passes through the bottom of the dish and appears at the upper surface, is wound several times round the iron cylinder, between the dish at its upper extremity and the wooden foot in which it is supported. The other extremity of the wire terminates in a small cup, *z*, as is seen in the figure.

A stout brass wire, one end of which is screwed firmly into the upper edge of the wooden dish, rises vertically about five inches in length, at which place it is bent at right angles, and continued in a horizontal direction over the axis of the iron rod. A

* I have made a number of experiments on small pieces, from the results of which it appears that much hammering is highly detrimental to the development of Magnetism in soft iron, whether the exciting cause be Galvanic or any other. And although good annealing is always essential, and facilitates to a considerable extent the display of polarity, that process is very far from restoring to the iron that degree of susceptibility which it frequently loses by the opperation of the hammer.

Cylindric rod iron of small dimensions may very easily be bent into the required form, without any hammering whatever; and I have found that small electro-magnets made in this way display the magnetic powers in a very exalted degree.

An electro-magnet of the above description, weighing three ounces, and furnished with one coil of wire, supported fourteen pounds. The poles were afterwards made to expose a larger surface, by welding to each end of the cylindric bar a square piece of good soft iron: with this alteration only, the lifting power was reduced to about five pounds, although the magnet was annealed as much as possible.

It appears to me that the superior magnetic energies displayed by these cylindric rods of iron, whilst subjected to the electric influence, are owing in a great measure to the peculiar fibrous texture which the metal is made to assume by the process which brings it to that peculiar shape.

† See Abstract 7th, page 37.

small brass wire screw, the upper end of which is inserted in a small cup, *c*, passes through the horizontal arm, and terminates with a hook at its lower end. A light copper wire, having a hook at one end, is by this means suspended to the hook of the screw. The inferior extremity of the pendent wire is pointed, and reaches into the wooden vessel, but not sufficiently low to touch its inner surface.

When an experiment is to be made with this apparatus, the extremities of the helical wire which passes through the dish and the lower cup, and also the extremity of the short wire which passes through the bottom of the upper cup, are to be well amalgamated. Mercury is now to be placed in the two cups for the convenience of connection, and also in the wooden dish, until the point of the pendant wire (which must also be amalgamated) dips slightly into it. By this means there will be formed a complete metallic connection between the mercury in the upper cup and that which is placed in the lower one.

If the connecting wire from the copper plate of a single Galvanic pair be now permitted to enter the upper cup, and that from the zinc to enter the lower one, *z*, then the direction of the electric current through the system of conductors will be from the upper to the lower cup. The cylindrical rod of iron, inclosed in the spiral, will become highly magnetic, and the suspended moveable wire will perform its revolutions round the upper pole.

With this arrangement, the *direction* in which the pendent wire performs its revolutions will entirely depend upon the character of the spiral, or upon the direction in which that part of the conductor passes round the iron rod from the upper to the lower extremity, and not upon any other circumstance whatever. For it being an established *law* in electro-magnetics, that *the direction of its revolutions is not altered* by simultaneously reversing the magnetic polarity and electric current; and, as in the present arrangement, the character of the magnetic poles in the extremities of the bar will at all times be determined by the direction of the electric stream, and that no vicissitude can possibly take place in the one without a corresponding and simultaneous vicissitude in the character of the other; it follows that the revolving wire will uniformly proceed in *one* and the *same* direction, whatever may be the nature of the electric stream; and that no vicissitudes in the direction of motion can possibly be accomplished by any similar arrangement.

Now, as the direction in which the wire revolves will be determined by the direction in which the spirals encompass the iron, it is plain that the apparatus may be so constructed as to perform its revolutions in any direction the experimenter may think proper to select: but the selection once made, and the standard spiral determined on, the apparatus becomes incapable of exhibiting that beautiful diversity of revolving motions which proceed from various combinations of the electric and magnetic forces.

If, however, the electric current were not to be continuous from one extreme cup to the other, the apparatus might then be made to perform the usual variety of electro-magnetic rotations; because, in that case, one current might be employed to give polarity to the iron, whilst another could be transmitted in any required direction through the pendent revolving wire.

Upon the same principle a bar of soft iron, properly connected, will rotate on its axis with an astonishing velocity; the direction of motion being always determined by the character of the helical conductor. The apparatus which I use for the exhibition of this interesting phenomenon, is similar to that by means of which I showed the rotation of a steel magnetic bar on its axis, by the influence of two electric currents;* the transient electro-magnet in this case being substituted for the permanent steel magnet in the other experiment. The manner in which the spiral is arranged will be understood by contemplating Fig. 2, Plate VI. The ends of the spiral are soldered to the cylindrical bar of iron, at the points A A, and to the centre is soldered a short wire with a descending point, for the purpose of maintaining an uninterrupted connection with an annular mass of mercury in which it revolves. The direction of motion is constantly the same whatever may be the nature of the Galvanic connections.

It does not appear that any very extensive experiments were attempted to improve the lifting powers of electro-magnets, from the time that my experiments were published in the *Transactions of the Society of Arts*, &c. for 1825, till the latter part of 1828. Mr. Watkins, Philosophical Instrument Maker, Charing Cross, had, however, made them of a much larger size than any which I had employed, but I am not aware to what extent he pursued the experiment.

In the year 1828, Professor Moll, of Utrecht, being on a visit to London, purchased of Mr. Watkins an electro-magnet weighing about five pounds—at that time, I believe, the largest which had been made. It was of round iron, about one inch in diameter, and furnished with a single copper wire twisted round it eighty-three times. When this magnet was excited by a large Galvanic surface, it supported about seventy-five pounds.

Professor Moll afterwards prepared another electro-magnet, which, when bent, was $12\frac{1}{2}$ inches high, $2\frac{1}{2}$ inches in diameter, and weighed about 26 pounds; prepared like the former with a single spiral conducting wire. With an acting Galvanic surface of eleven square feet, this magnet would support 154 pounds, but would not lift an anvil which weighed 200 pounds.

The success of these experiments, which established the first grand step in exalting the attractive powers of electro-magnets, gave a new impulse to the inquiry, which the American philosophers have pursued to an extent that will not be very easily

* See third Memoir, page 99.

surpassed. By dividing about 800 feet of conducting wire into twenty-six strands, and forming it into as many separate coils round a bar of soft iron, about sixty pounds in weight, and properly bent into the horse-shoe form, Professor Henry has been enabled to produce a magnetic force which completely eclipses every other in the whole annals of Magnetism; and no parallel is to be found since the miraculous suspension of the celebrated Oriental impostor in his iron coffin!

This electro-magnet is said to have supported nearly two tons when excited by about five square feet of Galvanic surface*—an extent comparatively trifling when compared to the prodigious magnetic force which it is capable of calling into action.†

The largest electro-magnet which I have yet exhibited in my lectures, weighs about sixteen pounds. It is formed of a small bar of soft iron, $1\frac{1}{2}$ inch across each side; the poles are separate from each other $1\frac{1}{4}$ inch; the cross piece, which joins the poles is from the same rod of iron, and about $3\frac{3}{4}$ inches long.

Twenty separate strands of copper wire, each strand about fifty feet in length, are coiled round the iron, one above another, from pole to pole, and separated from each other by intervening cases of silk: the first coil is only the thickness of one ply of silk from the iron; the twentieth, or outermost, about half-an-inch distant from it. By this mean the wires are completely insulated from each other without the trouble of covering them with thread or varnish. The ends of the wires project about two feet, for the convenience of connection.

With one of my small cylindrical batteries, exposing about 150 square inches of total surface, this electro-magnet supports 400 pounds. I have tried it with a larger battery, but its energies do not appear to be so materially exalted as might have been expected by increasing the extent of Galvanic surface. Much depends upon a proper acid solution; good nitric or nitrous acid, with about six or eight times its quantity of water, answers very well. With a new battery of the above dimensions, and a strong solution of salt and water, at a temperature of about 190° Fahr. the electro-magnet supported between seventy and eighty pounds, when the first seventeen coils only were in the circuit. With the three exterior coils alone in the circuit, it would just support the lifter, or cross piece. When the temperature of the solution was between 40° and 50°, the magnetic force excited was comparatively very feeble. With the innermost coil alone, and a strong acid solution, this electro-magnet supports about 100 pounds: with the four outermost wires, about 250 pounds. It improves in power with every additional coil until about the twelfth, but not perceptibly any further; therefore, the remaining eight coils appear to be entirely useless, although

* Silliman's Journal.

† In the year 1841, Mr. J. P. Joule, of Salford, a gentleman of great scientific attainments, produced an electro-magnet of an entirely novel form, and of a far superior lifting power to any other previously made. Its force of attraction was 2710 pounds, being 234 pounds to each pound of iron.—*Annals of Electricity, &c. Vol. VI.*

the last three of them, independently of the innermost seventeen, and at the distance of half-an-inch from the iron, produce in it a lifting power of seventy-five pounds.

It is evident from these results, that the exciting power of this miniature battery becomes improved by multiplying the number of conducting wires as far as twelve at least, although the greater part of them be at some considerable distance from the iron ; and it is highly probable, although the experiments which I have made on this bar do not satisfactorily prove the fact, that by employing a larger Galvanic surface, a much further addition to the number of conducting wires may be advantageously introduced into the circuit, for the excitation of the magnetic energies of soft iron. Perhaps the best arrangement would be to have a separate small battery to each wire.

Mr. Marsh has fitted up a bar of iron much larger than mine, with a similar distribution of the conducting wires to that devised, and so successfully employed by Professor Henry. Mr. Marsh's electro-magnet will support about 560 pounds when excited by a Galvanic battery similar to mine. These two, I believe, are the most powerful electro-magnets yet produced in this country.

A small electro-magnet, which I also employ on the lecture table, and the manner of its suspension, is represented by Fig 3, Plate VI. The magnet is of cylindric rod iron, and weighs four ounces : its poles are about a quarter of an inch asunder. It is furnished with six coils of wire, in the same manner as the large electro-magnet before described, and will support upwards of fifty pounds.

I find a triangular gin very convenient for the suspension of the magnet in these experiments. A stage, A A, of thin board, supporting two wooden dishes, C and Z, is fastened, at a proper height, to two of the legs of the gin. Mercury is placed in these vessels, and the dependent amalgamated extremities of the conducting wires dip into it—one into each portion. The vessels are sufficiently wide to admit of considerable motion of the wires in the mercury without interrupting the contact, which is sometimes occasioned by the swinging of the magnet and attached weight: the circuit is completed by other wires, which connect the battery with these two portions of mercury. When the weight is supported as in the figure, if an interruption be made by removing either of the connecting wires, the weight instantaneously drops on the table. The large magnet I suspend in the same way on a larger gin ; the weights which it supports are placed one after another on a square board, suspended by means of a cord at each corner from a hook in the cross piece, which joins the poles of the magnet.

With a new battery, and a solution of salt and water, at a temperature of 190° Fahr. the small electro-magnet, Fig. 3, supports sixteen pounds.

I noticed in some of my earliest experiments, that a bar of soft iron, which had been intensely magnetized by Galvanic action, retained a considerable degree of polarity when the exciting cause had been long removed—a phenomenon now more conspicuously

displayed by the employment of larger masses; nor does it appear to be an easy matter to subdue entirely this residual polarity. The poles may be reversed as frequently as we please, but still some, and frequently a considerable degree of polarity remains unneutralized. If the cross piece be permitted to remain attached to the poles when the Galvanic connection is broken, the residual polarity will still keep them together with an astonishing force, as is manifest by the very great weight which the magnet continues to support before the cross piece is disengaged from its poles. A residual polarity, however, still remains, but so enfeebled are its energies by the slightest interruption of polar contact, that the cross piece will seldom be supported a second time without a renewed excitation by the battery.

The vigorous residuum of polarity which retains the cross piece to the magnet, arises from a continued mutual attraction between the two; for whilst the battery operates upon the magnet, and excites it to action, the latter in its turn also excites the Magnetism of the iron connecting its poles, which iron becomes as decidedly polar as the magnet itself; a *north pole* being determined in that end of it which is in connection with the south pole of the magnet, and a *south pole* in the other end which is connected with the north pole of the magnet. The four magnetic poles thus brought into play and in vigorous operation on each other, will, if not separated, retain their positions at the points of connection, even though the first exciting cause be entirely withdrawn. But if the connection of the two pieces of iron be in the least interrupted, the Magnetism immediately recedes from the extremities, and becomes equally distributed in the metal; the vigour of the poles vanishes, and their Magnetism becomes totally incapable of keeping them attached to each other.

Precisely the same kind of reasoning will explain the cause of that well known deterioration of magnetic force which invariably takes place by removing the cross piece from a highly excited steel magnet. The excitation in this case, whether it be performed by the operation of a magnet, or by the more gradual process of adding small weights to those already suspended, is carried on whilst the poles and the cross piece are in contact; and both become excited at the same time. The magnet and cross piece now operate on each other with a gradually increasing vigour; the poles of the one becoming more and more energetic as the powers of the opposing poles of the other become further exalted. And this reciprocal increase of action will be exercised on each other till a maximum of mutual attraction is obtained, at which time the magnet will support its greatest load. If more weight be added, the mutual attractive forces of the magnet and cross piece will be overpowerd, and the two will separate. The Magnetism of the iron no longer displays polarity, and that of the steel partially recedes from its extremities; but in consequence of the retentive character of the metal, a considerable polarity is still displayed by the magnet: its energies, however, are very much diminished, and when the cross piece is replaced at the poles, the effort to

stimulate it to polarity is necessarily diminished also. The reciprocal attractions now operate with impaired forces, and consequently the load which can be supported is much less than before.

A compound steel magnet in my possession weighs nine pounds, which, when well magnetized, will support 120 pounds; but if weights be added till the cross piece falls off, the power is reduced to about seventy-five pounds.

Fig. 4 is a representation of an electro-magnetic sphere, mounted on a mahogany frame, consisting of a stout base board and two upright pillars, to the upper extremities of which is fixed a cross piece, or stage. On the centre of the stage, and directly over the sphere, is placed a dipping needle; and near to the extremities are inserted the lower ends of two wires, bent twice at right angles, as seen in the figure; the upper extremities of these wires are finely pointed, and support two horizontal needles.

The sphere, N S, is a cast-iron shell, about eight inches in diameter, and weighs sixty-eight pounds. Its surface is divided into three parts, which, for convenience, may be called *tropical* and *polar*, having one of the former and two of the latter. The tropical region is covered with four coils of copper wire in separate strata, insulated from each other, and also from the metallic sphere, by means of slips of silk; the exterior coil, being uncovered, is seen in the figure. The coils are prevented from slipping towards the poles by means of stout iron rings soldered to the sphere, one at each tropical circle.

The two sets of extremities of the conducting wires are soldered, one to each of the two copper disks or wheels, B B, through the centres of which the extremities of the horizontal axis of the sphere pass. The lower edges of the wheels dip into portions of mercury placed in two semicircular vessels, C C, which slide into the side of the pillars a little below the holes which receive the axis of the sphere.

By this arrangement the extremities of the wires which encircle the globe will constantly be in connection with the mercury in the two cups, in whatever position the poles may be placed. The connections with the battery are accomplished by two wires joining its poles, and the two portions of mercury in the vessels, C C.

If this apparatus be so placed, before the battery be attached, that the needles stand at right angles to the vertical plane of the frame, and the dip of the central needle be counterbalanced, they will be arranged nearly parallel to each other, as their distance prevents them being affected by the ball.

If the axis joining the poles of the sphere be now brought into a horizontal position, by turning the key, K, which enters the horizontal axle, the equator will be directly under the dipping needle. With this position of the apparatus, make the Galvanic connections so that the pole of the sphere which faces the north may assume north polarity, or that species of polarity which is natural to the northern regions of

the earth. The two horizontal needles will deviate from their former positions, having their north ends drawn towards the sphere: the central needle, being placed on the equator will remain undisturbed. If the north pole of the sphere be now gradually turned upwards, the horizontal needles will be directed still more towards this pole, and the north end of the central needle will incline. The angles of deviation and dip will increase more and more as the north pole of the sphere advances towards the zenith; and, when it has arrived at that point, those angles will have arrived at their maximum. The central needle will stand vertically, exhibiting a dip of 90° over the pole; and the two horizontal needles will point almost directly towards it. By permitting the sphere to resume its first position as gradually as it left it, the needles, which are influenced by it, will be observed to recede as gradually to their former positions.

If now the Galvanic connections be reversed, the whole of the needles become reversed also, because of a change in the polarity of the sphere; and the dipping needle, after a few oscillations, will settle in a horizontal position.

By experimenting in this way with various positions of the sphere, it will be found to operate on the needle in a manner highly imitative of the earth's Magnetism on different parts of its surface.

The two polar regions of the sphere exhibit a diffused polarity, the centres of which are nearly, perhaps exactly, in the poles of its equator. There are, however, in the polar regions, several points which exhibit distinct polarity; and, although of the same character as the general pole in which they are situated, they will draw a delicate needle held near to them from the direction which it takes when held at a greater distance from the action of the general or aggregate pole of that particular region. These local poles, I imagine, arise from a want of uniformity in the character of the iron. There are on the earth's surface aberrations of this kind arising from the local attractions of certain islands, and from causes not easily determined, which, as it happens, these accidental poles may serve to imitate.

W. S.

Woolwich, February, 1832.

RESEARCHES IN ELECTRO-MAGNETISM AND GALVANISM.

FIFTH MEMOIR.

Introduction.

Early in the spring of 1827, I repeated some of those highly interesting experiments brought forward by our illustrious Chemical Philosopher, the late Sir H. Davy, in illustration of his Bakerian Lecture, for 1826;* and before that time I repeated some others of a similar character, made several years ago by Francis Gybbon Spilsbury, Esq. of Cambridge.†

The repetition of these experiments naturally led to others, the results of which in some cases did not, in my opinion, appear perfectly reconcilable to the views which those philosophers had taken; notwithstanding which, having arrived at no decisive or satisfactory conclusions, I never ventured to publish an account either of the experiments themselves or of the theoretical views which were presented to my mind for an explanation of the phenomena they developed.

My attention, however, was again directed to this branch of philosophical research by a memoir which appeared in the second part of the *Philosophical Transactions*, for 1829, the title of which is "*An experimental Examination of the Electric and Chemical theories of Galvanism, by William Ritchie, A.M., F.R.S. Rector of the Royal Academy at Tain.*"

Mr. Ritchie has, in my opinion, very justly called in question the theories which have hitherto been adopted for the explanation of the nature of development of the energies of Galvanic arrangements; and he has certainly instituted some very curious and interesting experiments for the refutation of the doctrines they embrace. I do not, however, hold in the same high estimation the inferences deduced from those experiments; they appear to me to be exceptionable, and not of that decisive and satisfactory character which would seem best calculated to command universal assent. Fostering no partiality, however, to any particular hypothesis any further than I find

* Philosophical Transactions, for 1826, Part III.

† Transactions of the Cambridge Philosophical Society, Vol. II. Part I.

it reconcilable to facts, I regard those alone as true theories which comprehend an explanation of every variety of experiment.

As a Lecturer in this branch of philosophy, I of course feel a particular interest in inquiring into the character of every novel phenomenon which becomes developed by experiment, and also of becoming acquainted with the opinions of scientific men; notwithstanding which, I should consider it exceedingly unwise in any philosophical discussion to admit assertions which are not supported either by direct and unequivocal experiment, or deduced from the most rigid analogy.

Whilst reading Mr. Ritchie's memoir, I observed that the results of some of the experiments there detailed were not exactly conformable with those I had before noticed; I was, therefore, induced to examine them more particularly, and indeed to enter very minutely into the inquiry, in the course of which it became necessary to institute several new experiments, some of which have developed very curious and interesting facts: and, in consequence of their novel character, and the susceptibility which they promise of becoming available, both in a practical and theoretical point of view, it was my intention to have presented them to the public at a much earlier period; but, being engaged in other inquiries, I had not, at the time I was making those other experiments, an opportunity of arranging and extending such remarks on them as I considered they were entitled to.

Observing, however, that some philosophical speculations on this subject are again advanced in Dr. Brewster's *Journal of Science* for the present month (July), it appears to be one of immediate interest; and considering that the results of my researches, as far as they have extended, may possibly be found interesting to those who are participating in the inquiry, I have been induced to publish them as speedily as possible.

On re-commencing these researches, my first care was to repeat, with all possible attention, the experiments detailed in Mr. Ritchie's memoir; but it will be found in the sequel that, although some of the results which I have obtained are strictly confirmatory of what Mr. Ritchie has stated, yet others have proved to be decidedly equivocal—their character being determined by various conditions of the experiment. And it is rather singular that some of the results which I have observed are completely at variance with those stated to have been obtained from the same experiments by that gentleman.

As the results which I have noticed in some of those experiments may possibly tend to modify the claims which Mr. Ritchie's memoir professes to make on the attention of philosophers; and as the explanatory remarks which I may find necessary to offer might probably prejudice the opinions of those who may not have had an opportunity of reading that memoir, I have considered that I cannot in this place do a better service to the generality of my readers, or more justice to its author, than to

insert a copy of it for their perusal, in order that they may be the better enabled to appreciate the value of its evidence in discussing the theories of Galvanism.*

Examination of Mr. Ritchie's Memoir, with such Original Experiments as suggested and appeared interesting in illustrating the development of Galvanism and Electro-Magnetism; with Observations.

1. In researches of this character, the terms "*copper and zinc in the standard battery*," as adopted by Mr. Ritchie, are certainly more appropriate *criteria* than those of *positive* and *negative*, which are employed differently by different experimenters. They are terms which no one can mistake, whatever may be the nature of his Galvanometer; as a pair of those metals, properly connected with the instrument (as shown in Fig. 15, Plate VI.), and immersed in any of the diluted acids employed, form the "*standard battery;*" and the direction in which the needle moves establishes the criterion.

2. Mr. Ritchie has taken "for granted the truth of the experiment" on which the theory of Volta principally rests, but seems more partial to the opinion of Dr. Wollaston than to that of the Italian philosopher for an explanation of the phenomena developed; and he applies the same *chemical theory* in explanation of the energies displayed by the "*electric column*" of De Luc. Resuming his remarks on the experi-

* Copy of Mr. Ritchie's Memoir, extracted from the Philosophical Transactions, Part II. for 1829.

An experimental Examination of the Electric and Chemical Theories of Galvanism, by William Ritchie, A.M., F.R.S., Rector of the Royal Academy at Tain.—1. The continental philosophers still continue to adopt the electric theory of Galvanism proposed by Volta, whilst those in Britain as uniformly follow some modification of the chemical theory proposed by Dr. Wollaston. From this diversity of opinion we may safely conclude, that the experimental proofs for the truth of either theory are not sufficiently powerful to command the assent of all capable of appreciating the weight of such evidence. I have, therefore, ventured to lay before the society the following experiments and observations, as they appear to me to establish the truth of some modification of the chemical theory, and to demonstrate the fallacy of the principles on which the electric theory rests. 2. The fundamental principle assumed by Volta, and supported by his followers, is, that if dissimilar metals be brought into contact they are instantly thrown into opposite electric states. This he conceives to be a new law of nature, and claims to himself the honour of the discovery. He conceives that its truth is proved by the following experiment. Let *z* be a plate of zinc, and *c* a plate of copper, soldered together at the line of contact: hold the plate of zinc in the hand, and touch the under plate of a delicate electric condenser *(le condensateur a lames d'or)* with the copper plate, whilst a moistened finger is applied to the upper plate of the instrument. Remove the compound plate and the moistened finger, and then lift the upper plate of the instrument by the insulating handle, and the slips of gold leaf will be found to diverge. Taking for granted the truth of the experiment, the conclusion which Volta deduced from it by no means follows as a legitimate inference. Dr. Wollaston has shown that a Galvanic effect is produced by dissimilar metals, with the moist air of the atmosphere acting as a chemical agent and an imperfect conductor. The same fact is proved by the electric column of De Luc. The plate of zinc becomes partially oxidized by the oxygen of the atmosphere, Electricity is generated or set at liberty, and the film of moist air in contact with the two metals acts as a fluid conductor in an ordinary Voltaic arrangement. If the compound plate be coated with electric cement, to exclude the chemical action of the air on the zinc, I will venture to predict that no decided electric effect will take place. Until the supporters of the electric theory show, by direct experiment, that an electric effect does take place with this modification of the apparatus, we must view the whole of their reasoning as founded on gratuitous supposition. Having thus shown that Volta and his followers have overlooked what appears to me to be the very cause of the disturbance of electric equilibrium in the two metals, I shall now demonstrate that the other principle on which the theory is built is equally unfounded. This will appear obvious from the two following experiments.—*Experiment* 1. Having poured into a watch-glass

ment of Volta, he observes, " If the compound plate be coated with electric cement, to exclude the chemical action of the air on the zinc, I will venture to predict that no decided electric effect will be produced."

3. When a compound plate is covered with lac varnish (which, I presume, is as perfect an electric cement as can be desired), it is true the electric action is so far deteriorated as not to be easily detected in one compound plate only; but I must beg permission to observe, that if about six hundred of those compound plates, so covered with electric cement, be formed into a pile, with intervening pieces of paper, the combination is found to operate very well, displaying electric poles as decidedly as any electric column whatever. But, in consequence of the non-conducting medium with which the metallic pieces are sheathed, the energies of such a pile are much feebler than those of one composed of the same number of pairs, of similar metals, which are not so covered with varnish; and, so far am I from participating in the opinion that chemical action enhances the electrical energies of dry piles, that I believe, were it possible to exclude that action altogether, our electric columns would be benefited by it. In cases where the electric energies of dry piles cease to be displayed (which are not very frequent when the pile is properly constructed and preserved with care), the metals, which at first were simply in contact, are found to be disunited by a coating of oxide which has formed between them, or on those surfaces which

a quantity of diluted sulphuric acid, I placed on the surface of the fluid a piece of gold leaf, which was connected with one of the cups of a delicate Galvanometer. I then placed a disc of platina foil in the fluid below the gold leaf, and connected it with the other cup of the instrument: scarcely any electro-magnetic effect was produced. Having removed the acid, I substituted water containing condensed chlorine: a very decided electro-magnetic effect was produced. A similar effect was produced by using nitro-muriatic acid, or *aqua regia*, as it was formerly called, instead of the chlorine. The needle of the Galvanometer, in both cases, turned round in the same direction as it does when zinc was substituted for the gold leaf, and copper for the platina. Having tried, by the common method, the conducting powers of the diluted sulphuric acid and the water containing chlorine, I found that the diluted acid was the most powerful conductor. When the preceding experiment was repeated with discs of zinc and copper, instead of discs of gold and platina, I found that the most powerful effect was produced when the diluted sulphuric acid was used. This experiment clearly proves that the interposed fluid does not act merely as a conductor to the Electricity excited by the imaginary electro-motive force, since, in the first case, the Electricity generated is greatest when the conducting power of the fluid is least.—*Experiment* 2. Having made a small rectangular box, divided into two equal compartments by a diaphragm of bladder, I introduced into one of them a disc of hard copper, and into the other an equal disc of soft copper. These discs being connected with the cups of the Galvanometer, and the chambers filled with water, a considerable Galvanic effect was produced, and the needle turned round as it does when the place of the hard copper was supplied with a disc of zinc. I then poured a little nitrous acid into the chamber containing the hard copper, and observed that the effect was diminished; by adding a little more acid, the needle turned round several degrees in the opposite direction. This experiment completely overthrows the assumed principle, that the Galvanic effect increases with the conducting power of the fluid interposed between the metallic plates, since by increasing the conducting power of the fluid the effect was diminished, and by a proper increase was completely destroyed. It is a curious fact, that if nitric, sulphuric, or muriatic acid be used instead of the nitrous, the results will be quite the reverse. Having thus, I trust, satisfactorily shown that the electric theory is founded on false principles, I shall now very shortly examine the truth of the most generally received chemical theory of Galvanism. 3. Dr. Wollaston assumes that the positive Electricity is set at liberty by the combination of oxygen with one of the metals. This principle is frequently true, but in many cases it is totally false. This will be rendered obvious by the following experiment.—*Experiment* 3. Immerse two equal discs of zinc, connected by wires with the Galvanometer, into the chambers of the rectangular box formerly used, and fill both compartments with water: no action will, of course, take place.

were once in contact with each other; so that, by this coating of oxide, the contact of two pieces becomes discontinued, a slight interruption is thereby made in the arrangement, and the electric powers of the apparatus become diminished. When several pairs of metal become disjoined in the same manner, the energies of the remainder are unable to overcome the resistance which these multiplied interruptions place in the way; and consequently the electric powers of the pile cease to be exhibited. If the metals forming each pair were *soldered* together, so that no oxidation could possibly take place between them, it is not likely that a pile formed of elements so prepared would soon cease to display its electrical powers.

4. If a thin sheet of copper, covered completely on one side with tin, &c. be cut into small square pieces, or into discs, with a punch, a very efficient pile may be conveniently formed by these compound pieces, and intervening discs of writing paper. And a pile so formed, and preserved in a dry place, will never cease to display its electric energies until the two sides of the compound pieces have ceased to be two distinct metals.

First Experiment.

5. In examining this experiment, the results which I have obtained are precisely the same as those stated in the Mr. Ritchie's memoir.

Pour a little sulphuric, nitric, or muriatic acid into one of the chambers: a considerable Galvanic effect will be produced, and the needle will turn in the same direction as it does when copper is substituted for the plate of zinc immersed in the chamber containing water alone. This agrees with the chemical theory. Again, instead of the above acids, use nitrous acid, and the the needle will turn round in the opposite direction. The same thing holds good when discs of copper or iron are employed. This is completely at variance with the chemical theory, since that plate is negative, or corresponds with copper in the standard battery, on which the greatest chemical action of the fluid takes place. The following experiment is also hostile to the generally received theory.—*Experiment* 4. Having taken two pieces of block tin, I cut the surface of one of them into ridges by means of a three-cornered file, so that the surface was doubled. With these two pieces I formed a binary combination, and immersed them in a diluted nitro-muriatic acid. A very considerable electro-magnetic effect was produced, and the needle turned round in the same direction as it does when a plate of zinc is substituted for the plain disc in the standard battery. It is obvions that there must be a greater chemical action between the acid and the furrowed plate than the other; and yet the furrowed plate corresponds with copper in the standard battery on which the greatest chemical action takes place. The results in the following experiment were also unexpected.—*Experiment* 5. Take equal pieces of soft zinc, copper, iron, or brass; beat one of each pair on a smooth anvil until they are as hard as possible. Form a binary combination with pairs of the same metal, and use diluted sulphuric acid, and it will be found, by the Galvanometer, that the hard metal in each case corresponds with zinc in the standard battery. If two pieces of steel be employed, one of them soft and the other tempered, a Galvanic effect will be produced, but of a contrary character. The soft steel will correspond with zinc, and the hard with copper, in the battery of comparison. The result of the following experiment seems also at variance with previous notions on the subject.—*Experiment* 6. Having procured two small iron bars, with the ends made bright with a file, and copper wires connected with the other ends, I heated one end of one of them, connected the wires with the Galvanometer, and then immersed the hot and cold ends in water: a considerable action took place, and the cold iron was found to correspond with zinc in the standard battery. Since oxygen combines more rapidly with hot than with cold iron, positive Electricity ought, according to the received opinions, to have appeared at the hot iron, whereas the contrary was actually the case.—From the short view which I have taken of this interesting subject, it appears that the electric theory is quite unfounded, and that the chemical theory will require some modification to embrace the facts contained in the last experiments. This I shall not, however, attempt at present—as my object in this paper is rather to demolish old fabrics and collect new materials, from which a more substantial edifice may be raised.

6. I participate in the opinion "that the interposed fluid does not act merely as a conductor to the Electricity" of the two metallic plates: I am persuaded that it has other and very important functions to exercise in the Galvanic process. Its conducting property, however, must not be regarded as unessential in that process, for it appears by several experiments that the conduction of the fluid medium is highly influential in facilitating the display of Galvanic and electro-magnetic phenomena. And, as is well known, *it is a necessary condition in all Galvanic arrangements that the whole of the circuit be of conducting materials.* When the above experiment was made with copper and zinc, it cannot be imagined that "the most powerful effect," being "produced when the diluted sulphuric acid was used," was any proof of the inefficacy of the conducting power of the fluid medium in promoting the Galvanic process; but rather, it might be inferred, that the *superior* conduction of that acid solution operated favourably to the greater development of Electricity.

7. The energies and direction of electric currents, in Galvanic combinations, have an obvious dependence upon the electrical relations of the elements employed. Gold and platina, by simple contact, display very feeble electrical energies indeed; and, perhaps, both metals have so nearly the same tendency to yield up their Electricity to a solution of sulphuric acid, that the difference of force is too trifling to generate any other than a very feeble stream; and, even with nitro-muriatic acid, the electrical powers displayed are exceedingly feeble.

8. With copper and zinc it is quite different: the copper yields up its Electricity so rapidly and abundantly to the zinc, that it may be detected by its attractions, in specimens of those metals much smaller than a sixpence, by one single contact only; and few Galvanic combinations display more energetic electric powers than these metals do when associated with any of the acid solutions.

9. That the electrical relations which subsist amongst the elements of a Galvanic circle operate, and that very powerfully, in giving direction to the electrical current I am persuaded very few will deny who are practically acquainted with the science. Many experiments strengthen the supposition, and many others might be devised to prove the potency of their influence; but it would seem as if the ardour of contention for the support of particular and favourite theories already in repute had prevented philosophers from observing those influential powers essentially embraced in the true one. I do not, however, suppose with Volta, that the Electricity developed by simple contact of the metals exercises the extent of influence in Galvanic arrangements which that philosopher imagined. I have interposed, in various ways, pairs of copper and zinc, of two square feet in surface, in the Galvanic circle, without observing the least effect in modifying the energies displayed by a *pair of wires* of the same metals which were placed in an acid solution. And I have proved by experiments which

will be described in the sequel, *that electro-magnetic powers may be displayed without any metallic contact whatever.*

Second Experiment.

This experiment was made with two pieces of copper, the one hard by hammering, the other quite soft. The pieces which I employed were two inches long and one inch broad: they were placed one in each compartment of a vessel divided in the middle by a diaphragm of bladder. The acids employed were the nitrous, nitric, sulphuric, and muriatic. The metals exposed about two square inches of surface each to the action of the fluid. As this experiment consists of several parts, it will be necessary to examine them separately.

10. *With Nitrous Acid.*—With diluted nitrous acid in one chamber and water in the other, the results will depend upon circumstances which Mr. Ritchie seems to have entirely overlooked, but which, in my opinion, are too important to be permitted in this place to pass unnoticed. When the acid is diluted with forty or fifty times its quantity* of water, the piece of copper, whether *hard* or *soft*, which is immersed in that diluted acid displays its Electricity in the character of zinc in the *standard battery*, and consequently the other piece, which is in the water, operates as copper by the same criterion.

11. If the acid be very little diluted—not more for instance than with an equal quantity of water—the electrical characters of the two metallic pieces become changed, and that which is placed in the acid operates in the capacity of copper in the *standard battery.* But, even in this case, there are other circumstances attending the results which must be particularly noticed before we can become properly acquainted with the nature of the action.

12. When the diluted acid is placed in one chamber and water in the other, the two bright pieces of copper uniformly exhibit the same electrical relations to each other at their first immersion, whether the diluted acid be strong or feeble, and that piece which is placed in the acid operates as zinc in the *standard battery ;* and it is not before they have remained some time in their respective situations that their electrical relations change. This change takes place when the acid is not much diluted, and when it is capable of producing a material change on the surface of the metallic piece on which it operates. With feeble acid, I have never observed any such vicissitude in the electrical characters of the two pieces, although, in several cases, they have been purposely left in their respective chambers for twenty or thirty minutes; and the Electricity was still appreciable even at the end of that time—the piece which is immersed in the acid uniformly displaying the same electrical character as zinc in

* Quantity is here, and in every other experiment in this work, estimated by measure.

the *standard battery*. Hence we may conclude, that in those cases where the pieces of copper undergo a change in their electrical relations, *that change is effected by the chemical action of the acid on the surface of one of them.*

13. Mr. Ritchie presumes that "this experiment completely overthrows the assumed principle, that the Galvanic effect increases with the conducting power of the fluid interposed between the metallic plates." Were I attempting to support this part of the Voltaic theory, I am not certain that I could have selected an experiment more to my purpose. When the pieces of copper are placed in water only, I have not detected any Galvanic action. And I must also observe, that with water in one chamber and very much diluted acid in the other, the electrical energies displayed are very feeble; but, as the proportion of acid becomes greater, the electrical powers are exalted; and when the "conducting power of the fluid interposed between the metallic plates" is still farther improved by a few drops of acid in the water contained in the other chamber, *those powers are always displayed to the greatest advantage.*

14. *With Nitric Acid.*—When this acid is employed, precisely the same phenomena are observed as when the experiment is made with the nitrous, and the only difference in the process consists principally in the proportions of acid and water. The electrical characters of the two pieces of copper undergo a change with a much less proportion of the nitric than of the nitrous to the water, and it must be very much diluted to prevent that species of mutation for any length of time, for the more energetic the chemical action the sooner is that change accomplished. It takes place with either piece in the acid, but sooner, and more effectually, when the soft one is subjected to its action and the hard piece in the water. The electrical energies are, however, more energetic when both portions of fluid are acidulated, observing always that one of them must be very slightly so.

15. *With Sulphuric Acid.*—With this acid I have obtained precisely the same results as those observed by Mr. Ritchie. The piece of copper, which is placed in the acid solution, operates in the capacity of zinc in the *standard battery*; and there does not appear to be much difference in the amount of effect, whether it be the *hard* or the *soft* piece of copper that is placed in that solution. The action is very lively at first, and continues with considerable force for several minutes; but no change of electrical relations could be produced by any power of the acid, or by long-continued action. In this, as in the two former parts of the experiment, the electrical energies are exalted by slightly acidulating the water in the other chamber.

16. *With Muriatic Acid.*—With this acid, as with the rest, the electrical energies are exalted by improving the conducting power of the fluid in both chambers. The piece of copper, whether *hard* or *soft*, which is placed in the acid not much diluted, uniformly operates as zinc in the *standard battery*; and this electrical relation to the piece in the water slightly acidulated, is always displayed whether the metals be clean or not.

17. There are some very curious phenomena developed by the employment of muriatic acid, which are well worthy of attention, and which seem unfavourable to the pretensions of the chemical theory of Galvanism. When the pieces of copper have remained some minutes in their respective chambers, the piece in the strongest acid solution becomes covered with a black sooty oxide, and when washed in clean water, the surface is still tarnished of a deep dirty brown colour; but the other piece which was in the water slightly acidulated, comes out quite bright and clean. If now, whilst the two pieces are in these states, they be again immersed in their respective chambers, the needle will be deflected 90°, and sometimes 100°—the piece of metal in the stronger acid still displaying the character of zinc in the *standard battery;* and the angle of deflection will continue much greater, and for a considerably longer period than if both pieces had been bright before immersion.

18. If the piece which is in the water slightly acidulated be taken out and well washed and cleaned, whilst the other remains in the stronger acid, the moment it resumes its proper situation the needle is again violently deflected in the same direction as before, and will continue steady for several minutes at a considerable angle; but if the other piece be taken out of the acid and made quite bright, whilst that in the water remains in its place, on the bright piece being returned to its station no such sudden deflection takes place: the angle of deflection is generated gradually and slowly, and the needle never arrives at the point at which it stands by the former processes.

19. Now, according to the pretensions of the chemical theory of Galvanism, the contrary to all these appearances ought to have taken place, and the bright piece would have been expected to display its Electricity in the character of zinc in the *standard battery* the most intensely at the first immersion, which is contrary to fact. And so far do the results in some cases operate against the dictates (there are no *principles* established) of that theory, that the bright piece, when immersed even in the stronger portion of acid, will frequently exhibit its Electricity as copper in the *standard battery*, and gradually and slowly resume the opposite state as it becomes more covered with oxide. Similar phenomena are observed when the experiment is made with sulphuric acid.

20. When the nitric or nitrous acids are employed with copper, it may perhaps, at the first impression of the mind, appear a very singular circumstance that the piece which is immersed in the strongest portion, and on which the greatest chemical action is exercised, should operate as copper, whilst the other, on which the chemical action is very feeble, should display the character of zinc in the *standard battery.* But we are astonished at these apparent anomalies only from the dominion which we have permitted particular theories to exercise over the imagination—theories which have not become favourites from any knowledge which we absolutely possess of their correctness,

but because they have emanated from men of acknowledged talent and penetration; a recommendation at all times powerful and authoritative, and to some minds almost irresistable, but which, in scientific pursuits, should always be adopted with caution, whatever degree of respect may be due to their authors. If we could but divest our minds of philosophical prejudices, and permit our reasonings to rest upon those data only which experiment presents to our observation, we might then hope to arrive at a knowledge of the true theory of Galvanism, which will explain all those perplexing difficulties and apparent anomalies which we at present meet with in this branch of science.

21. Sir H. Davy, who has on many occasions successfully interrogated nature, and has so skilfully associated the opinions of Volta and Wollaston in his ingenious electro-chemical theory of Galvanism, seems to have totally failed to acomplish an explanation of the phenomena I have last noticed.

22. In the Bakerian Lecture, for 1826, Sir Humphry observes, "In combinations in which weak and strong solutions of acids are the two fluids, both being of the same kind, the electrical action is usually feeble," and "the result usually depends upon the nature of the solution. If oxide is formed and deposited, the strongest acid is negative* with respect to the diluted one."

23. When "oxide is formed and deposited," I apprehend that the oxide is understood to be deposited on the surface of the metal undergoing, or which has undergone, the greatest chemical action, or on that piece which is placed in the stronger portion of diluted acid; and that, when it has become covered with the deposited oxide, the chemical action will abate and eventually will be overpowered by the action of the feeble acid on the other piece which is not so covered by the deposition of oxide. I can frame no other meaning to this part of Sir Humphry's lecture, neither can I suspect for a moment that I have put on it a wrong construction when I find, in the same lecture, that "the destruction of the positive surface by the chemical negative agent is regarded as a necessary condition" "of the electro-chemical theory."

24. When copper is exposed to the action of nitric acid, diluted with two or three times its quantity of water, the oxide formed is not deposited on the surface of the metal, which remains bright, but is immediately taken up by the acid solution, which becomes of a bluish green colour by its impregnation with the dissolved copper. But if the acid be diluted with fifty or sixty times its quantity of water, the copper placed in it soon becomes tarnished by a deposition of oxide on its surface.

25. Now, with copper and these different powers of the acid solution formed into a Galvanic combination, the results are as I have before stated. The piece in the *strong acid solution*, the surface of which undergoes *rapid destruction* but remains *quite bright*, very soon assumes the character of copper in the *standard battery*, whilst the other

* Sir H. Davy has applied the term positive to the zinc part, and negative to the copper part of a Galvanic battery.

piece operates as zinc, or is the *positive* metal, notwithstanding the *feebleness of the chemical action*, and consequent *tarnished surface* of the metal by the *very slightly acidulated water*. And it appears from every observation which I have made on this unexplained phenomenon, that *the more rapid the destruction* of that piece which operates as copper, *the more energetic* is the Electricity developed by the experiment.* These facts, which I have shown cannot be explained by the electro-chemical theory, must necessarily depend upon the operation of some principle, or principles, which that theory does not embrace—the development of which is most likely to be accomplished by a rigid interrogation of the elements of Galvanic combinations of the *simplest possible form*.

Third Experiment.

26. The same vessel with two chambers was again employed in this experiment, having diluted acid in one compartment and water in the other. The metals were copper, zinc, and iron; two pieces of each exactly alike, and those which I employed were exactly of the same size as the copper in the last experiment. The acids were the nitric, nitrous, muriatic, and sulphuric.

27. Although Mr. Ritchie has comprehended all these varieties in one experiment, there are absolutely several, and very interesting experiments, to be contemplated under this head by combining the materials as he has directed. I have shown in the last experiment that the results, except in *degree*, are the same whether the *hard* or *soft* piece of copper is placed in the strongest portion of diluted acid; and, therefore, that part of this experiment in which copper enters into the combination may be regarded as a mere repetition of the former, and may be dismissed at once by referring the reader to the second experiment.

28. *Zinc with Nitric and Nitrous Acid.*—When two pieces of zinc are employed with either of those acids, the results are precisely the same, and are similar to those in which copper is employed with the same acids. The phenomena which are

* For the present I shall make but very few observations on this part of the electro-chemical theory, more than that of presenting to the reader's notice a problem or two worthy of consideration.

If "the destruction of the positive surface by the chemical negative agent is regarded as a necessary condition" "of the electro-chemical theory," how does it happen that

"Rhodium, irridium, and gold act in combinations consisting of acid and alkali, on which they have *no chemical effect*, exactly like platinum—the surface of the metal in the solution of alkali being *positive*, that in the solution of acid *negative?*"

Or that "the chemical changes produced in combinations of this kind are best observed in cases where the metals undergo *no change?*" (Bakerian Lecture, 1826.)

These instances of Galvanic action which Sir Humphry has brought forward are not, in my opinion, very favourable to the theory in question; indeed they are sufficient evidence of themselves to prove that the "destruction of the *positive* surface" is not a necessary condition in Galvanic arrangements.

displayed will depend upon the strength of the acid solution: if very feeble, the piece placed in the water will exhibit its Electricity in the character of copper in the *standard battery*, and will retain that character for a long time together; but, if the acid solution be pretty strong, the same appearances will at first be observed as with a weak solution, but the electrical relations of the two pieces of zinc will soon change; and finally, the other piece, which is exposed to the action of the acid, will display Electricity in the capacity of copper in the *standard battery.* But the results are much more certain, and the Electricity displayed far more energetic, when both portions of interposed fluid are acidulated. Two or three drops of acid will be sufficient in the chamber containing the water.

29. The foregoing experiment may, perhaps, be considered as one of those the phenomena of which are supposed to be explained by the deposition of oxide on the surface of the metal; but I cannot see that the same explanation will apply to the phenomena developed by the following variation with similar elements. Into a wine glass full of water, pour four or five drops of good nitric acid. Prepare two pieces of sheet zinc with conducting wires, and exactly alike in polish, hardness, size, and figure. Place one of them in the acid solution; if bubbles of gas flow gently upwards from its surface, the solution is of a proper strength. This piece of metal having remained in the liquid about a minute, will have its surface tarnished. If now the bright piece be introduced, and both in connection with the Galvanometer, the needle will be deflected 10° or 15°, indicating the piece which last entered the acid solution to be operating in the character of copper in the *standard battery*, and an equilibrium will not be restored till several minutes have elapsed. If now the other piece be taken out and again made bright, leaving its fellow piece in the acid solution, the moment the polished piece enters the combination it will display its Electricity in the character of copper in the *standard battery*, and continue in that state for some time. The same thing holds good when nitrous acid is employed.*

30. *Zinc and Muriatic Acid.*—When diluted muriatic acid is placed in one chamber and water in the other, the results with zinc are of the same character as with copper and that acid. When both pieces are immersed whilst bright, that which is placed in the water uniformly displays its Electricity in the character of copper in the *standard battery.* There does not appear to be so great a tendency to change this electrical relation with the other piece, as is observed when two pieces of copper are employed with this acid; notwithstanding which, the electrical energies may be materially modified in precisely the same way, for if that piece only which is in the

* It is necessary in these researches to be careful in having the two pieces of metal exactly of the same electrical character; and as zinc is one of those metals the Electricity of which becomes considerably modified by *size, figure, asperities, &c.* as well as by *brightness*, it will be necessary to attend to those particulars, otherwise much uncertainty and perplexity may attend the results. It is, therefore, prudent to have several pieces in readiness, and to select *two* which, when formed into a combination and connected with the Galvanometer, deflect the needle to a *less angle* than any other pair amongst them.

water be taken out and made *quite bright*, when again introduced the needle will be driven through an arc of 50° or 60°, the bright piece operating in the capacity of copper in the *standard battery*. But if the other piece which is in the diluted acid be taken out and prepared in the same way, when again introduced to its place in the circuit no such sudden deflection takes place, and the angle which the needle subsequently makes is generated very slowly, and it never becomes so great as in the former case. Hence it appears that in whatever way the experiment my be varied, the bright piece will have a tendency to operate as copper in the *standard battery* in precisely the same way, though not so energetically, as I have noticed when copper and muriatic acid are employed. If the water in one chamber be not acidulated, this tendency will be displayed to a certain extent; but the needle, although considerably checked in the violence of its deflection, and will even remain motionless for a second or two after placing the bright piece in the acid solution, the electrical energies are too feeble by this process to move the needle in the opposite direction. But if both portions of the interposed fluid be acidulated, the one more powerfully than the other, this phenomenon never fails to be exhibited.

31. Let the two chambers be supplied with diluted muriatic acid of different degrees of strength; in one chamber for instance acid diluted with eight times its quantity of water, and in the other acid with twelve or fourteen times its quantity of water. Place in these acid solutions two pieces of zinc of exactly the same size, figure, temper, and polish, being at the same time in connection with the Galvanometer; the needle will be deflected in the direction which indicates the piece which is placed in the stronger solution to be operating as zinc in the *standard battery*. Take the piece which operated as copper out of the feebler solution, wash and wipe it clean, and make it bright with fine new glass paper; introduce it again into the same chamber, and the deflection will be in the same direction as before, but the angle will be much greater and continue a longer time. When the metals have remained some time in their respective places, take out the other piece and make it bright in the same way; on being returned to its situation the needle will be deflected the other way, showing that the bright piece, even in the stronger acid solution, operates as copper in the *standard battery*, and consequently the other piece on which the *feebler* chemical action is exercised, operates as zinc by that standard. This phenomenon is still more strikingly displayed by immersing both pieces of zinc in the same acid solution, and proceeding as I have already directed. The bright piece invariably displays the same electrical character as copper in the *standard battery*, and at the first plunge into the liquid will sometimes deflect the needle over an arc of 100°; and will maintain an angle of more than 20° for several minutes when the acid solution is not very strong.

32. Sir H. Davy, who observed similar phenomena with copper and hydro-sulphuret solutions, say, " I have often found the order which I have mentioned of metallic copper

being positive with respect to copper that had been a few seconds in solution of hydro-sulphuret reversed in a singular and capricious way, but on investigating the cause I found that the copper was tarnished; and on heating any kind of polished copper strongly, so as to produce a thin coating of oxide anywhere on its surface, it becomes strongly negative to copper plunged in solution of hydro-sulphurate: the same effect was produced by the action of acids. (Bakerian Lecture for 1826.)

33. The fact thus observed by Sir Humphry is unquestionably one in point, though the explanation attempted is by no means satisfactory, nor is the phenomenon, in my opinion, easily reconciled with the doctrine which that lecture was intended to support, The phenomenon is not " capricious," but *constant* and *uniform*. It is *displayed regularly under the same circumstances* and never deceives the experimenter. It is *not momentary*, but continues long even *fifteen* or *twenty* minutes, and in favourable circumstances a much longer time elapses before an equilibrium is restored. And so little does its display depend upon *tarnish*, in the sense used by Sir. Humphry, that the electrical powers operate on the needle with great energy and promptitude when the piece last plunged into the solution is as *bright* as possibly it can be made.*

34. *Zinc and Sulphuric Acid.*—Little more can be advanced, in illustration of the phenomena developed by a Galvanic combination of these elements, than what I have stated whilst speaking of zinc with muriatic acid; they are of precisely the same character, but the Electricity displayed is more active. The piece in the acid solution operates as zinc to that placed in the water, which consequently becomes of the same electrical character as copper in the *standard battery*. But in this, as in every other case, the electrical energies are much exalted by slightly acidulating the water, showing that conduction is an essential element in Voltaic arrangements.

35. With two pieces of zinc and a weak solution of sulphuric acid, the phenomenon of the bright piece operating as copper in the *standard battery* is beautifully displayed: the contrary takes place with sulphuric acid alone, or with a strong solu-

* Sir Humphry himself seems to have discovered that *tarnish* was not necessarily concerned in producing this singular effect, for in the following paragraph to that from which I have already quoted he observes, that "There are some singular circumstances connected with the violent and intense chemical action of copper on solution of hydro-sulphurets which are worthy of being described. When a piece of copper connected with the multiplier (or Galvanometer) has been for a minute in strong solution of hydro-sulphuret of potassa, on introducing a piece of polished copper connected with the other wire, there is often a violent and momentary negative charge communicated to it, which sends the needle through a whole revolution; it then oscillates and almost immediately returns, and takes the direction which indicates that the piece first plunged in is negative. This effect continues for some minutes, then becomes weaker; at last the two sides are in equilibrium, and the piece which was first plunged in now becomes positive with respect to the other. The first described of these effects seems to depend upon the discharge of the clean copper by the negative Electricity accumulated by the contact of the plate first plunged in, before the relative states produced by the metallic contact and the regular currents occur; and the second, to the detaching or peeling off of the coat of sulphuret, which has the effect of exposing a clean surface, and which effect is probably occasioned by the oxidation of the positive side of the plate." By perusing the above paragraph, the reader will readily perceive that there is no mention of *tarnish* being on the " piece of polished copper," the " violent and momentary negative charge" of which sends the needle through a whole revolution of the Galvanometer.

tion. With a strong solution of common salt and water and a few drops of sulphuric acid, I have never found it to fail; and it seems to answer best when the coating of oxide accumulated on the surface of the plate first immersed in the solution is not too thick, and the other piece as bright as possible. From this circumstance I was willing to admit, that, if it could not be satisfactorily proved to the contrary, it was possible that a weak acid solution already in chemical play with the piece first plunged in, might require some time before the same degree of chemical energy could be exercised on the bright piece which was plunged in afterwards; and by thus reasoning upon the principles of the chemical theory, the latter piece ought for a moment to operate as copper in the *standard battery.* I might indeed have been satisfied to the contrary, by having observed that the phenomenon is not of a very transitory character, but is displayed with great steadiness—in some cases for more than half an hour; during which time, and perhaps from the first moment the bright piece was immersed, the chemical action upon it must have become equal to, or greater than upon the other piece. But in order to determine the weight of this reasoning still more decidedly, I devised the following variation of the experiment.

36. When one of the pieces of zinc had been sufficiently long in the acid solution, it was taken out, gently wiped and dried, by which means the coating of oxide on its surface became hard and black. This piece, and one which was quite bright, were properly connected with the Galvanometer; which done, both were plunged into the acid solution, and immediately the needle was deflected, indicating the bright piece to be operating as zinc in the *standard battery.* I repeated it several times with precisely the same results, which would seem to be favourable to the idea I had formed of the nature of the process; but how far this experiment will operate in favour of the chemical theory of Galvanism I am not so prepared to determine; for if this theory requires, as a necessary condition, that the surface of the *positive metal* (zinc in the *standard battery*) should undergo a *more rapid destruction* by the action of the acid than the other, or *negative metal* (copper in the *standard battery*), I imagine many experiments may be advanced which are completely hostile to that doctrine. I shall mention one in this place which can very easily be made, and which, in my opinion, will transmit conviction to the mind of the most prejudiced theorist.

37. Let two equal slips of sheet zinc, of any convenient size, be polished with glass paper. Let the surface of one of them be amalgamated, by spreading mercury over it with a piece of clean rag, so that it may become quite brilliant.* Both pieces being furnished with connecting wires, and in proper communication with the Galvanometer, let them be plunged into a weak solution of either sulphuric or muriatic acid; the amalgamated piece will operate as zinc, and of course the other piece as copper, in the *standard battery.* But it will be observed, particularly if the com-

* Zinc may be easily amalgamated by first dipping it into a solution of sulphuric acid and afterwards into mercury.

bination be placed in a glass vessel, that the piece which operates as copper undergoes *rapid destruction*, whilst the other is scarcely affected by chemical action. Gas will copiously ascend from the former, whilst a few indolent bubbles only will be observed on the latter, which cling to its surface without making their escape. Hence it appears from this experiment, that the most oxidizable metal in a Galvanic combination does not universally operate as zinc in the *standard battery*, or in other words, it is not always the *positive metal.**

I know of no experiment that operates more decidedly against the chemical theory of Galvanism than the one I have last described; the Electricity displayed is uniform and steady from beginning to end; and its duration is determined by the durability of the *negative piece*, (copper in the *standard battery*,) and not by the other as that theory supposes. I have observed a deflection of the needle of more than 10° for two successive hours with two pieces, each exposing about one square inch of surface to the action of the acid solution; at the end of which time the needle was perfectly steady at that angle, although the piece which operated as copper was nearly destroyed. On examining the amalgamated piece, very slight traces only of chemical action could be observed on its surface. The same amalgamated piece was successively combined with two others, which it likewise outlasted, still operating as zinc in the *standard battery*, and was not even then much decayed by chemical action, but had become exceedingly brittle by combining with the fluid metal.†

* Sir H. Davy, in his Bakerian Lecture for 1826, observes, that "zinc in amalgamation with mercury is *positive* with respect to pure zinc;" hut he is perfectly silent as regards the nature of the chemical action which is developed hy a combination of these materials in any acid solution, though one might have supposed that, had he made the experiment, this striking and singular phenomenon could not have escaped the attention of so penetrating an ohserver, nor have heen permitted to pass unnoticed whilst discussing the theories of Galvanism.

Sir Humphry has also stated, that "there is not any inherent and specific property in each metal which gives it the electrical character; it depends upon its peculiar state—on that form of aggregation which fits it for *chemical change*."

From this statement I imagine that Sir Humphry has made no experiment like that described in the text, for it never could have been discovered from that experiment, nor from any other with which I am acquainted, that the amalgamation of zinc *exalts* its *oxidability* in a solution of either sulphuric or muriatic acid; the *most essential* "*chemical change*" required to satisfy the conditions of the electro-chemical theory.

† Were it not on account of the brittleness and other inconveniencies occasioned by the incorporation of the mercury with the zinc, amalgamation of the surfaces of zinc plates in Galvanic hatteries would hecome an important improvement; for the metal would last much longer, and remain bright for a considerahle time, even for several successive hours—essential considerations in the employment of this apparatus.

Notwithstanding the inconveniences, however, the improvement afforded hy amalgamating the surfaces of zinc plates hecomes availahle in many experiments; for the violent and intense chemical action which is exercised on zinc by a solution of sulphuric or muriatic acid, with the consequent evolution of heat and annoying liberation of hydrogen, have no place when the plates are amalgamated: the action is tranquil and uniform, and the disengagement of gas, which is trifling, occurs only when the circuit is complete and at the surface of the copper plate. The electric powers are highly exalted, and continue in play much longer than with pure zinc; and the only care of the experimenter is to prevent the copper, or whatever metal he suhstituted, from becoming amalgamated.

With a solution of nitrous acid, the electrical energies of two pieces of zinc, the one pure and the other amalgamated, are displayed in a very superior degree; hut, in consequence of the amalgamated piece hecoming partially oxidized, and liherating gas at its surface, the experiment is not so decisively opposed to the chemical theory of Galvanism as when either muriatic or sulphuric acid is employed.

38. *Iron and Nitric Acid.*—When diluted nitric acid is placed in one chamber and water in the other, the piece of iron which is immersed in the acid solution operates as copper, and consequently the other piece which is placed in the water displays its Electricity in the character of zinc in the *standard battery.* When a few drops of acid are mixed with the water, the electrical energies become very much exalted, and the needle will frequently mark an angle of 35°, particularly if the stronger portion of the acid solution be not very feeble; and these energies seem to improve with an increase of acid in that portion of the fluid. These electrical relations of the two pieces appear to be constant with every power of the acid solution, even from the first immersion, which is a peculiarity in this experiment that I have not observed either with copper or zinc, for when those metals are separately employed with nitric acid of different degrees of strength, the piece which is immersed in the stronger

Two pieces of rolled zinc, each presenting ten square inches of surface, one of which was amalgamated and made quite brilliant, were formed into a Galvanic combination, with nitrous acid diluted with twelve times its quantity of water, and connected with the Galvanometer; the needle, after several oscillations, reposed at an angle of 65 degrees, after which the following results were observed, without in the least disturbing the apparatus :—

Minutes.	Degrees.	Minutes.	Degrees.
In 5 after the first immersion	70	" 75	50
" 20	68	" 90	45
" 30	65	" 105	40
" 45	60	" 120	35
" 60	55	In four hours	25

The metals were not disturbed for fifteen hours afterwards, at the end of which time the needle marked an angle of 18 degrees. An interruption was now made in the circuit, without disturbing the metallic plates, when the needle had reposed in the magnetic meridian the circuit was again completed, and the needle deflected to an angle of 30 degrees, and became steady at 19 degrees.

The electrical powers displayed by two pieces of zinc, the one pure and the other amalgamated, and a solution of nitrous acid, are sufficiently energetic to produce electro-magnetic rotations even on a pretty large scale. With similar pieces to those employed in the preceding experiment, the following amongst other electro-magnetic experiments may be exhibited.

The apparatus in Fig. 5, Plate VI. represents an inverted horse-shoe magnet, which may be supplied with any kind of stand to keep it in a vertical position. A bent wire is supported on one of the magnetic poles by means of a fine point, on which it can move freely, whilst its depending extremities dip slightly into mercury which is placed in a circular wooden trough, through the centre of which is an opening for the admission of one of the magnetic poles. The trough being adjusted to a proper height, is to be kept firmly to the magnet by means of a binding screw : through the side of the trough passes a brass wire, the extremities of which are to be amalgamated for the purpose of preserving contact—one with the mercury contained in the trough, and the other, by being bent upwards and passing through the bottom of a small cup, will communicate with another portion of mercury there placed. A cup, also containing mercury, is placed on the bend of the pendent wire, and there fixed by means of the upper part of the short wire which forms the pivot passing through its bottom part, and that extremity of the pivot being amalgamated unites with the mercury in the cup.

If now the plate of pure zinc (which operates in the capacity of copper in the *standard battery*) be connected with the upper cup, as represented by the broken wire, c, and the amalgamated plate (which operates as zinc in the *standard battery*) with the lower cup by the broken wire, z, there will be a complete metallic communication between the two plates, and the circuit will be divided into two branches from the upper cup, down the two arms of the pendent wire to the mercury contained in the circular trough. The electrical fluid will, therefore, by this arrangement be transmitted through the pendent wire in the direction indicated by the arrows; and, by virtue of the electric and magnetic powers, the wire will revolve with great celerity round the magnetic pole. If the connecting wires be made to change places, the electric stream will be transmitted upwards in the two arms of the revolving wire, and the direction of motion will also change. If the apparatus be placed on the other magnetic pole, the wire will perform its revolutions in a reverse order with similar connections.

solution first displays its Electricity in the character of zinc in the *standard battery*, and afterwards changes to that of copper, which is not the case with two pieces of iron, for they uniformly display the same electrical relations from the beginning to the end of the experiment.*

39. When one piece of iron has been exposed for a short to the action of a feeble solution of nitric acid, it will operate as zinc to another bright piece which is plunged in afterwards—the latter operating in the character of copper in the *standard battery*; but this species of action with iron is of very short duration, and the needle almost *immediately* returns to that direction, which marks the last piece immersed to be operating as zinc in the *standard battery*.

40. *Iron and Nitrous Acid.*—The electrical relations of two pieces of polished iron when placed in two portions of this acid very differently diluted, or the one piece in the acid solution and the other in water, are precisely of the same character as when the nitric is employed, but the electrical energies displayed are more energetic. I have experimented with the acid and water in a variety of proportions, and the results are uniformly of the same character: the piece which is placed in the acid solution operates as copper, whilst that in the water, whether acidulated or not, displays the electrical character of zinc in the *standard battery*.

41. When both pieces are placed in the same acid solution, the one a minute or two before the other, the latter, if bright, operates as copper in the *standard battery* very powerfully indeed; and this singular phenomenon is exhibited to as great an advantage with these materials as with any that I have employed. But these electrical relations very soon cease, and the pieces almost immediately display Electricity in the opposite way precisely the same as when nitric acid is employed. There is also another phenomenon exhibited in these experiments which, I believe, has never before been noticed, but which, by the regularity of its display, must necessarily involve some *theoretical principle*, and consequently becomes as interesting to the philosopher as any other. I have observed it more or less in several other experiments; but, as it is very decidedly exhibited with these materials, I will describe it in this place.

42. When two pieces of polished iron have been, for a few minutes, immersed in a weak solution of nitrous acid, and in connexion with the Galvanometer, if one piece be taken out and very soon returned to its place, the needle will be deflected to a considerable angle, amounting in some instances to 90°—indicating the piece last

* It is difficult to imagine how it were possible that Mr. Ritchie should so far mistake the nature of the phenomena in this part of the experiment, since there appears no ambiguity whatever involved by employing the nitric acid variously diluted, as is the case when either copper or zinc is employed; besides, the electrical powers are displayed so energetically that one might have thought no mistake could possibly have occurred. I have frequently obtained an angle of deflection of 45 degrees when both portions of the interposed fluid were acidulated, the one very slightly and the other tolerably strong; and I have performed electro-magnetic rotations with a pair, each piece of which exposed two square inches only to the fluid media. It may, perhaps, be necessary to mention that the pieces which I have generally employed were the best English iron, but I have employed others with similar results.

plunged in to be operating as copper in the *standard battery*, and the needle will not return so quickly as if that piece had been bright before immersion, but will frequently continue deflected for some time. The same thing takes place with either piece: it is a matter of no consequence which is first plunged into the acid solution, the last will always occasion a display of the phenomenon I have mentioned. I have obtained the same result for twenty successive times, by first taking out one piece, then the other, leaving them in the solution about half a minute between each time.

43. *Iron and Muriatic Acid.*—When two equal pieces of iron are immersed in a solution of muriatic acid, the piece last plunged in will display its Electricity in the character of copper in the *standard battery.* If now the other piece be taken out, it will also operate in the same capacity in precisely the same manner as when iron and nitrous acid are employed; and this species of action takes place when the pieces are immersed, the one in the acid solution and the other in water, so that the first effect indicated by the needle will depend on the order of immersion; but if they be left unmolested for a minute or two, the piece in the acid solution will operate as copper, whilst that which is surrounded by water will operate as zinc in the *standard battery.* And these electrical relations of the two pieces will be uniformly displayed while undisturbed in their respective chambers; but if either piece be in the *least moved* in the fluid, that piece will immediately operate as copper in the *standard battery.*

44. This singular and curious phenomenon, which I believe has not before been noticed, I shall endeavour to describe with some degree of minuteness; and likewise the process by which it appears to be the most decidedly exhibited. Let two flat pieces of good iron, having each about two square inches of surface exactly alike, and well polished, be connected with the Galvanometer, and placed in a vessel containing muriatic acid diluted with two or three times its quantity of water: the needle will vibrate a little, but will soon come to rest. But as it is next to impossible to select two pieces of iron so nearly alike in their electrical characters as not to display some Galvanic effect, it is likely that the needle will not repose in the magnetic meridian, but will make some small angle therewith: let that piece only which the needle indicates to be operating as zinc in the *standard battery*, be gently moved in the interposed fluid; the needle will immediately be deflected the contrary way, showing that the electrical relations of the two pieces of metal become changed by this process. When the needle has again come to rest, move the other piece, permitting the first moved piece to remain unmolested: the needle will again change its direction, and will indicate the last disturbed piece to be operating as copper in the *standard battery.*

45. If, whilst the connections are complete, one of the pieces be moved rapidly to and fro in the acid solution, whilst the other remains at rest, the needle will be deflected to an angle of 40° or 50°, and may be kept steady at about 20° by continuing

the motion, still indicating the moving piece to be operating as copper in the *standard battery;* but the moment the motion has ceased, the needle returns to the meridian, and very frequently takes a position on the other side.

46. I have tried iron with solutions of other acids, but cannot discover that decided effect as with the muriatic. I have also tried if the same phenomenon could be exhibited by employing other metals, such as copper, zinc, brass, &c. in different acid solutions, but I have failed to obtain anything like that precision of results which are afforded by iron and diluted muriatic acid. In some cases, indeed, the same process appears to operate in the contrary, way and particularly with tin in a solution of nitro-muriatic acid, as will be more particularly noticed in the sequel.

47. *Iron and Sulphuric Acid.*—I shall have very little to advance under this head, as the phenomena displayed are precisely of the same character as when muriatic acid is employed. There is not, however, so decided an effect produced by agitating one of the pieces as in a solution of muriatic acid, but the result by that process is of the same nature.

48. From a retrospection of this complicated experiment, it will be observed that the results which I have obtained are very different from those stated in Mr. Ritchie's memoir. Perhaps some of those differences may have emanated from a dissimilitude in the mode of experimenting, or from that gentleman not having noticed those peculiar phenomena which I have minutely described; but there are certainly some discrepances which, I am persuaded, no allowance of that nature can possibly reconcile.*

Fourth Experiment.

49. On repeating this experiment, I have, under *certain circumstances*, observed the same results as those stated by Mr. Ritchie; but there are *other circumstances* under which the electrical characters of two pieces of tin, the one furrowed and the other smooth, in a Galvanic combination with diluted nitro-muriatic acid may be considerably varied; and even in those cases where the furrowed piece invariably assumes the character of copper in the *standard battery*, it does not acquire that property by virtue

* There is an advantage in separating the two portions of fluid by a bladder partition, which is not afforded by placing the two pieces of metal in separate vessels connected by moistened asbestos, as was the practice with Sir H. Davy: for it is a well known fact, that the *nearer* the metals approximate each other in the interposed fluid, the *greater* is the Galvanic effect; and when the electrical energies are very feeble it is necessary to give every facility to the display of the phenomena. Hence it will be found that when both pieces *press gently* against the opposite side of the bladder, the needle will be deflected to a greater angle than when they are placed at a greater distance from each other. If unacidulated water be employed in one of the chambers, this precaution will be necessary to be attended to.

This circumstance alone appears to me highly favourable to the opinion, that the energies of a Galvanic combination are *exalted* in the same proportion as the *conducting power* of the fluid medium becomes *improved*; for by shortening the distance between the two plates in the acid solution, a portion of impeding obstacles to the transmission of Electricity becomes removed, and the electrical stream flows more copiously and with greater celerity by the facility thus afforded, which in fact amounts precisely to the same thing as if the distance were *constant* and the *conduction* of the acid solution *improved*.

of its exposing the *larger surface*,* but has that quality communicated to it in consequence of the *peculiar configuration* of that surface.

50. If two pieces of tin, the one furrowed all over its surface with a file, with sharp edged ridges between the furrows, and the other piece quite plain and smooth, be placed in nitro-muriatic acid, diluted with three or four times its quantity of water, and properly connected with the Galvanometer, the furrowed piece, whether the *greater* or *smaller* of the two, will display the same electrical character as copper in the *standard battery*. I have found this to be the case when the plain piece exposed a surface to the acid solution *ten times* the extent of that exposed by the furrowed piece. The furrowed piece displays the character of copper when combined with a smooth piece in a strong solution of nitric, nitrous, or muriatic acid, and also in a strong solution of nitro-sulphuric acid.

51. If the two pieces be placed in nitro-muriatic acid very much diluted, the furrowed piece appears to lose the above-named property, or at least its electrical powers in the capacity of copper become so far deteriorated in a feeble solution of this compound acid, that other circumstances operate which cause it to assume the electrical character of either copper or zinc in the *standard battery*.

52. When two pieces of tin of the same size, whether *rough* or *smooth*, form the Galvanic pair, and are placed in a weak solution of equal parts of *nitrous* and *muriatic* acids, the one which exposes the *brighter surface* operates in the capacity of zinc in the *standard battery*. If one piece be taken out of the solution and made bright by scraping with sharp edged glass, on again being introduced into the acid it operates as zinc in the *standard battery*. With *weak solutions* of equal parts of *nitric* and *muriatic acids*, the *bright piece* operates in the contrary way; but the phenomena developed by experiments of this nature are displayed in the most satisfactory manner by having both pieces plain and smooth, and of the same size.

53. When the nitro-muriatic acid is composed of equal quantities of *nitrous* and *muriatic* acids, the bright piece operates as zinc in solutions of every degree of strength. The same law holds good in *strong solutions* when the *nitric* is employed with muriatic; but in weak solutions of this compound acid, the bright piece operates in the capacity of copper in the *standard battery*.

54. Let two bright pieces of tin, equal in size and figure, be connected with the Galvanometer, and placed in a mixture of equal portions of *nitric* and *muriatic* acids, diluted with twenty times its quantity of water, and observe which of the pieces operates as zinc; and, when they have remained two or three minutes, take out that particular piece, wipe it clean, and scrape it quite bright with glass: when again introduced to the Galvanic circle, it will operate as copper in the *standard battery*.

* If a disproportion of surfaces were the only condition necessary in this experiment to satisfy particular *theoretical* views, why not employ those surfaces in the same state of polish, and not have recourse to the file on the one any more than the other?

Repeat the process with the other piece, which will also display its Electricity in the character of copper in the *standard battery.* If the compound acid be diluted with four times its quantity of water only, the bright piece will operate as zinc by the same process.

55. With a solution of *nitric* acid, even pretty strong, the bright piece, by the above process, operates as copper very energetically. The first effect, however, which takes place at the moment the bright piece touches the liquid is a sudden start of the needle, indicating the bright piece to be operating as zinc; but the needle by the first impulse never becomes deflected more than about 3° or 4°, and is so suddenly and energetically deflected the other way, that it frequently marks an angle of 50° or 60°. The first effect is of so momentary a character that it requires particular attention to perceive it; it is, however, a beautiful theoretical phenomenon, and can always be detected if the connections be made before the piece touches the fluid medium. It appears to be a sudden discharge of Electricity, as was the opinion of Sir Humphry Davy (see note, page 134), but does not appear to be of the same character as that described in Articles 31 and 33, for *that* has some degree of *permanency*, but *this* has none. But the second effect in this experiment, which indicates the bright piece to be operating in the capacity of copper, is precisely of the same character as that described in those articles. I have observed those sudden starts of the needle in several other experiments: they occur in different directions with different combinations.

56. Dr. Brewster has endeavoured to explain the results of Mr. Ritchie's fourth experiment, by supposing that it only requires "that the *aqua regia* employed should have an excess of nitric acid to put the plate of metal in a condition to act as a negative body—that the nitric acid should be in quantity sufficient to form a portion of oxide on the surface. The whole plate in this case will be negative in regard to another clean plate, and the greater the surface, that is, the grooved one will be negative in regard to the plain one."—(*Edinburgh Journal of Science*, for July, 1830.) This doctrine accords with that of Sir Humphry Davy (Article 22 and 23), and is one proof at least that I have not misunderstood our late illustrious chemist; but I cannot see how it will explain the phenomena described in Article 35, for in that experiment the *only acid* employed was the *nitric*, and when one of the pieces was completely covered with oxide, it became the *positive* and not the *negative* piece; unless we take into account the first minute and sudden discharge which I have described. The deposition of oxide in regulating the electrical characters of the two pieces will not apply to Mr. Ritchie's experiment, for in those cases where the grooved piece operates as copper, it will maintain that character from the first moment the two pieces enter the acid solution, and also if plunged in after the smooth piece is partly covered with oxide. In consequence of the furrows on its surface, a greater deposition of oxide is, however, sometimes formed on it than on the surface of

the other piece; but this deposition of oxide, instead of determining its character as copper in the *standard battery*, frequently determines it in the other way (zinc in the *standard battery*): this circumstance happens only when the two pieces have been exposed for a considerable time to the action of the acid. The grooved piece, in consequence of the mechanical impediments presented by the ridges on its surface, prevents the oxide from falling down and, therefore, soon becomes clogged with it, which deposition of oxide serves in some measure to protect it from chemical action. The plain piece on the other hand, having no such coating of oxide, is more assailable by the acid solution, the consequence of which is, it becomes more corroded than the furrowed piece, and presents a multitude of asperities on its surface which no file can imitate; it therefore becomes the *negative* piece, and *no deposition* of oxide on the surface of the other will change its electrical character. The fact is, the phenomenon displayed belongs to a distinct class, the cause of which is *a difference in the configuration of the surfaces of the two pieces;* and when other causes do not interfere *that* which presents the *most asperous surface*, becomes the *negative piece* (copper in the *standard battery.*) To the same class of phenomena belong those which are displayed by two pieces of copper or zinc in strong and feeble solutions of nitric or nitrous acid (Articles 11, 12, 14, 27, 28), and perhaps those also which are developed when iron is employed with the same acids in different degrees of solution (Articles 38 and 40.) In those experiments, and in many others, the piece *most corroded*, and consequently *most asperous*, becomes the *negative metal* (*copper* in the *standard battery.*)*

* Tin is one of those metals two pieces of which, however nicely selected, are generally in different states of Electricity, and their electrical characters are easily detected by the Galvanometer. If cast at different times from the same mass, and even into the same mould, the Galvanic effect of two pieces is generally considerable. I have obtained an angle of 20 degrees, steadily kept up for several minutes, from small specimens cast under these circumstances; and even from the same casting, if a piece be cut in two and both parts made so nearly alike that no difference as regards size, figure, polish, &c. can possibly be detected, when those parts are formed into a Galvanic pair a difference in their electrical relations may generally be detected by the Galvanometer. Hence it will readily appear how careful an experimenter ought to be in selecting specimens to ascertain the true character of the phenomena displayed, by treating one of them as Mr. Ritchie has proposed.

When two flat pieces, each two inches long and one inch broad, has been ascertained to be very nearly alike in their electrical characters while of the same figure and polish—one of them was grooved all over its surface. A binary combination was now made of the rough and smooth piece, and the proper connections made with the Galvanometer; the combination was placed in a proper vessel, and nitro-muriatic acid, diluted with four times its quantity of water, poured in. The following observations were made for a quarter of an hour:—The first pouring in of the acid solution

		Degrees.			Degrees.
	The deflection was	26	In 8	Minutes........	20
In 1	Minute	30	" 9	....	14
" 2	Minutes........	30	" 10	....	12
" 3	....	30	" 11	....	12
" 4	....	30	" 12	....	11
" 5	....	30	" 13	....	10
" 6	....	28	" 14	....	10
" 7	....	25	" 15	....	10

And the needle stood at 10 degs. for several minutes longer, still indicating the grooved piece to be operating in the character of copper in the *standard battery*. I have made several experiments of this character with similar results; but, for the reasons stated in the text, the pieces when long used sometimes change their electrical relations.

Fifth Experiment.

57. The most striking and interesting phenomena developed in this experiment are those displayed by two pieces of zinc, the particles on the surface of which are in different degrees of compactness.

When two pieces of cast zinc, one only of which has been hammered, are formed into a binary combination, they become a very active Galvanic pair; and, for the same reason, a very energetic combination is formed by employing one of the pieces *rolled* and the other *cast*, although the former be the *softer* of the two: hence it cannot be supposed that the different electric capacities in which they operate is a consequence of their different states of *hardness* or *pliability*, as Mr. Ritchie imagines; but is a most decided instance of the influence of asperities in determining the electrical character of metals in Galvanic combinations, as the following experiments will amply demonstrate:—

58. Two pieces of zinc, each exposing two square inches of surface, the one *cast* and quite *hard* and *brittle;* the other *rolled*, but sufficiently *soft* and *pliable* to wrap round the figure like a strip of sheet lead—were placed in a glass vessel, and connected with the Galvanometer by proper conducting wires; sulphuric acid was poured in, and a small deviation of the needle was observed, indicating the rolled piece to be operating as zinc in the *standard battery*. Water to the amount of about three times the quantity of acid was now gently poured in: the chemical action became excessive, and the needle was deflected 30° in the same direction as at first. The following were the results of five minutes, which was the whole time that the pieces remained in the acid solution:—

	Degrees.	
First pouring in of the water,	30	The rolled piece constantly operating as zinc in the *standard battery*.
In 1 minute,	38	
" 2 minutes,	38	
" 3	37	
" 4	33	
" 5	30	

59. The metals being now brushed in clean water, were again introduced to the same acid solution: the same electrical relations of the two pieces were again displayed for five minutes, the angle of deflection being steady at 12°.

60. The same pieces were again brushed in clean water, and the experiment repeated with a fresh portion of sulphuric acid, diluted with three times its quantity of water: the metals were connected with the Galvanometer, and the results again observed for five minutes, which were as follows:—

	Degrees.	
First plunge	180	The rolled piece uniformly displaying its Electricity in the capacity of zinc in the *standard battery.**
In 1 minute steady at . . .	45	
" 2 minutes, ...	47	
" 3	40	
" 4	35	
" 5	32	

61. Experiments were instituted for the purpose of examining the electrical relations of two pieces of the same kind of *cast* zinc, the one hammered to level the asperities on its surface, but afterwards softened by annealing. The slips were parts of the same plate which was cast for the purpose, and prepared as above stated. When properly connected, and introduced into sulphuric acid, diluted as before, the deflection of the needle amounted to 30°, but gradually decreased as the surface of the hammered piece became rough by corrosion, but continued for five minutes in the direction indicating the *soft*, *hammered* piece to be operating in the character of zinc in the *standard battery*.

62. When the pieces of zinc were again cleaned with a brush and water, the hard piece was made rough by a rasp, and both were again placed in the diluted acid used in the last experiment: the needle was first deflected in the direction indicating the rasped piece to be operating as zinc in the *standard battery*, which was a mere consequence of its superior brightness and partial leveling of the natural asperities by the rasp, for the needle was immediately deflected the other way, and after a few oscillations it stood firmly at an angle of 15°, indicating the *soft*, *smooth* piece to be operating as zinc in the *standard battery*, or in the same electrical character as it had done in the former experiment with these two pieces.

63. The experiment was varied by hammering the same piece again, in order to flatten the asperities which the action of the acid had produced on its surface, and afterwards annealing it to render it *soft* and *pliable*, the other piece still remaining *brittle* and *rough*. When placed in their proper situations in sulphuric acid, diluted with three times its quantity of water, the Electricity displayed was as powerful as at first, and precisely of the same character.

* In experiments with these materials, the chemical action is excessively intense for the first three minutes, about which time it generally subsides very rapidly. The electrical energies appear to diminish from three causes :—1st. As the chemical action subsides, the height of the fluid medium subsides also, and therefore a less portion of metallic surface is exposed in the latter than in the former part of the experiment.—2nd. The longer the pieces of zinc are exposed to the action of the acid, the more equal they become in the asperous character of their surfaces, consequently their electrical energies are the most vigorous when the rolled piece is quite smooth or well hammered to level the asperities on its surface, which experience demonstrates.—3rd. Since, by the first part of the experiment, *decomposition* is rapidly produced, a portion of the constituent parts of the fluid medium becomes either *determined* in the circuit or entirely *expelled;* the latter part, therefore, proceeds under very different circumstances, both as regards *quantity*, *quality*, and *arrangement* of the elements employed. This latter cause is common to all Galvanic arrangements.

64. When the acid is diluted with thirty or forty times its quantity of water, the Electricity displayed is much feebler than with stronger acid, but the metals uniformly exhibit the same electrical character, and the needle remains steady for a considerable time.

65. No one who becomes acquainted with these decisive results can imagine that the *temper* of the metals had any part in varying their electrical relations. Some other cause must evidently exist, and which, I am persuaded, will be found in the irregularity of their surfaces; for it appears that, in whatever way the experiments are varied, the piece which presents the smoothest surface to the diluted acid displays its electric powers in the capacity of zinc; whilst the other piece, which presents an infinitude of asperities, either from its natural texture or by artificial preparation, uniformly becomes of the same electrical character as copper in the *standard battery.*

66. In a practical point of view, we readily ascertain, by these experiments, that there must be a considerable difference in the power of two Galvanic batteries of equal size when the zinc employed in their construction is in different states; for it is very evident that as rolled zinc operates very powerfully in the capacity of zinc when combined with another piece which is cast, but neither rolled nor hammered, plates of *rolled zinc* will have an advantage over *cast plates* when combined with copper in the usual way. Hence, when the Galvanic arrangement becomes very extensive, amounting to several hundred pairs, the additional power obtained by employing *rolled zinc* must be very great indeed.

67. With a view of becoming acquainted more particularly with the increase of power obtained by employing rolled zinc in Galvanic batteries, I compared the two kinds with a piece of copper, trying first the rolled zinc and then the cast: this could easily be accomplished by having the combination loose in the glass vessel, and the pieces not tied or otherwise fastened together, by which means the two pieces of zinc could be changed at pleasure, without removing the piece of copper. Diluted sulphuric acid was employed in the experiment, and the advantage gained by the rolled zinc amounted to about 10°, when the angle of deflection was 30° by the cast piece.*

68. There is another circumstance which attends Galvanic batteries in which cast plates are employed, that appeared easily explicable by these discoveries. It is well known that those batteries lose their power to a considerable extent by long use, although the zinc plates remain pretty thick; but no explanation of the cause, that I am aware of, has yet been attempted, and the circumstance is known as a mere experienced fact. The zinc becomes very much corroded and pitted on the surface;

* The Galvanic batteries which I employ in my lectures, have long been admired for the great power which they display in proportion to their size. I had attributed their superior energies to the close approximation of the pairs to each other, a circumstance well known to enhance the power of this apparatus; but the experiments already detailed have opened other views, and disclosed the most probable cause of their efficacy. I have always employed thin sheet zinc, and in every inch of the battery I have five pairs of the Cruickshank form.

and, according to the view which Mr. Ritchie has taken of the chemical theory in the remarks on his fourth experiment, the surface of the zinc becoming expanded by the irregularities, and consequently exposing a larger space to the acid, the chemical action would be augmented in proportion, and the battery ought to operate *better* than at first, which is contrary to fact; for whatever may be the increase of chemical action on the zinc plate by this extension of its surface, the power evidently becomes diminished, and in some cases is so very far deteriorated that no excess whatever of exciting acid will restore the battery to its original powers.

69. Finding by the experiments I have already detailed, and by many others which were made at a much earlier period,* that asperities on the surface of the plates have a great influence in determining their electrical relations, I considered that the same cause might possibly operate in reducing the power of Galvanic batteries, by the

* I was first led to this class of experiments from a train of reasoning which originated whilst repeating some of the experiments detailed by Mr. Spilsbury in the *Cambridge Philosophical Transactions,* vol. 2, part I. On repeating some of the experiments described in the Bakerian Lecture, for 1826, my attention was again directed to the influence of asperous surfaces, and also of dull and bright surfaces, in determining the electrical character of metallic bodies. Early in the spring of 1827, I constructed several dry *electric colums* precisely upon these principles, having each one kind of metal only; and I have at present by me one which was made at that time, and which is still in good action; it consists of 600 pieces of zinc, and will be particularly described in another part of this work. In consequeuce of not having published these discoveries (which I consider belong exclusively to myself) in any other way than in my lectures, they are, in some of the Journals of Science, erroneously attributed to M. De la Rive and Mr. Watkins. The former gentleman became acquainted with my experiments in a conversation with the latter, to whom the communication respecting them was made by myself. The conversation between M. De la Rive and Mr. Watkins happened in June or July, 1828, more than a year after my zinc pile was first constructed. Mr. Watkins informed me of the particulars of the conversation, and in a few weeks afterwards I received from him the following note:—

"Dear Sir,—In the *Literary Gazette* of to-day, there is an extract from *Le Globe* of some communications from M. Augustus De la Rive respecting the primary source of Electricity in a Voltaic battery, or dry pile. He states that he has successfully repeated the experiments of an *English Chemist,* who produced Electricity by means of a pile composed exclusively of zinc, one face of each plate of which was rough and the other polished, and so on. This is in consequence of what I named to him. I regret he has not mentioned your name, as I most particularly gave him it. I send you this note as I have a recollection of something you said about inserting in the forthcoming *Philosophical Magazine* a communication respecting dry piles.

"Yours truly,

"Saturday." "Francis Watkins.

The following is the article alluded to, extracted from the *Literary Gazette,* page 542, August 23rd, 1828:—

"*Metallic Electricity.*—M. Aguste De la Rive, of Geneva, has constantly observed, that the action produced by the component parts of a Galvanic pile has ceased either when they were placed in a vacuum or in a medium which did not act chemically upon them. On the other hand M. De la Rive has successfully repeated the experiments of an English Chemist, who produced Electricity by means of a pile composed exclusively of zinc, one face of each plate of which was rough, the other polished. These plates, which, placed at a certain distance from each other, had no communication except by means of the ambient air, nevertheless exhibited a considerable degree of Electricity. The consequences which result from these two experiments, with respect to the idea we ought to form of the principal cause of the development of Electricity in the Galvanic pile, are evident, and appear to us to be of a nature to modify the notions of the learned world as to one of the most important facts of natural philosophy.—*Le Globe.*"

From my personal acquaintance with Mr. Watkins, I am perfectly satisfied that he is a gentleman incapable of committing anything like plagiarism; and I imagine that the mistake has originated through M. De la Rive not having recollected my name. In the *Literary Gazette,* for September 20th, 1828, appears my letter to the Editor on this subject, to which I must refer the reader, as it would occupy too much room for insertion in this place.—W. S.

surface of the zinc plates becoming rugged and covered with innumerable asperities, from their frequent exposure to the action of the exciting acid. With a view of determining this problem by experiment, I procured of my friend Mr. Marsh some old zinc plates which had been for a long time in a Galvanic battery, and were so completely corroded by the action of the acid with which they had formerly been excited, that they exposed surfaces as irregular and rugged as need to be wished for in experiments of this nature ; and if asperities were the only conditions necessary to modify the electrical character of zinc, it was expected that these old plates would operate in the capacity of copper when combined with others newly cast. Accordingly, I formed several pairs of old and new pieces, and I have examined the nature of their electrical relations in a great variety of experiments, some of which I shall now describe as minutely as possible.

70. The first experiment was rather preliminary than otherwise to the series I had proposed. An old plate, such as I have described, was combined with one newly cast. When properly connected, and placed in the glass vessel with sulphuric acid, the *old Galvanic plate* operated as copper, which was shown by a deflection of the needle of about 3°: when water to the amount of about half the quantity of acid was poured in, the deflection was much greater, and in the same direction. Water was again added until it amounted to about twice the quantity of acid: the deflection increased, and for a while appeared to verify what I had anticipated ; it soon, however, became reduced, and in two minutes the electrical character of the two pieces changed, and the needle was deflected through an angle of 20° the other way, and continued so for about a minute, when the chemical action became so excessive that it was necessary to take the metals out of the diluted acid.*

71. The plates were now well washed and brushed in clean water, in order to remove as much as possible the oxide which was formed on their surfaces. When they were placed in sulphuric acid, diluted with six times its quantity of water, the *old piece* resumed the capacity in which it operated at the commencement of the last experiment, displaying its Electricity in the character of copper with great energy and steadiness, maintaining an angle of deflection of 20° for five minutes.

72. As the electrical relations of the metals in the last experiment were displayed whilst in a feebler solution than was before employed (70), and as those relations had undergone *no change* in the latter (71), it became necesssry to repeat the experiment as at first, in order to ascertain if the additional water was the cause of that immutability; or if the same electrical relations would be exhibited with other proportions of acid and water, when the metals were freed as much as possible from oxide,

* The *old plate* was not washed, or otherwise cleaned, prior to the experiment, and therefore the change which was observed in the electrical characters of the two pieces was probably effected by adhering oxide, for no such mutation was again noticed in any experiment with these two pieces.

without disturbing the asperities on their surfaces. The pieces were, therefore, well brushed in clean water, connected with the Galvanometer, and placed in sulphuric acid: the angle of deflection was about 2°, indicating the *old piece* to be operating as copper in the *standard battery*. Water was poured in as at first (70), and the angle of deflection increased to 25°. These pieces were afterwards tried in acid solutions of various degrees of strength; but, in whatever proportions of acid and water they were placed, the *old Galvanic plate* uniformly operated as copper in the *standard battery*.

73. With a view of changing the electrical relations of the two pieces, the *new one* was grooved all over one of its flat surfaces, and both were cleaned with a brush and water: when placed in sulphuric acid, the *new piece*, which had been grooved, operated in the character of copper in the *standard battery*, and maintained that character, though very feebly, for five minutes, at the expiration of which time both pieces were removed from the acid.

74. The *new piece* was now grooved a little deeper than before, and both again placed in sulphuric acid: the *new piece* again operated as copper in the *standard battery*. When water was poured into the acid, the angle of deflection increased; and when the quantity of water amounted to twice that of the acid, the needle marked an angle of 20°, and was steady for five minutes, still indicating the *new piece*, which had been grooved on its surface, to be operating as copper in the *standard battery*.

75. The *new piece*, which by this time had become much corroded, was now grooved on both sides, and deeply serrated all round its edges; both pieces were brushed in clean water, and placed in sulphuric acid: on the connection being completed, the grooved piece again displayed its Electricity in the character of copper in the *standard battery*. When the acid was diluted with three times its quantity of water, the needle became deflected to an angle of 45°, and was perfectly steady for about one minute; in two minutes the angle was reduced to 37°, but never became less than 35° for eight minutes, at the termination of which time it was suddenly reduced to 15°. On taking the metals out of the vessel, I found that a part of the grooved piece had dropped off, and that part of the remainder, which was below the surface of the fluid, was nearly destroyed by the action of the acid; but even in this state, and when not more than half the size of the other piece, it still maintained its electrical character as copper in the *standard battery*. Similar results have been obtained with solutions of nitric, nitrous, and muriatic acids.

76. This remarkable property communicated to pieces of zinc by having their surfaces grooved or otherwise irregular, has been strikingly displayed in every experiment with these two pieces, and proves most decidedly that the influence of asperities operates to a very great extent in deteriorating the energies of those Galvanic batteries,

the *cast zinc* plates of which have become much corroded by exposure to strong acid solutions. I have been the more particular in detailing, with some degree of minuteness, the experiments with *rough* and *smooth* zinc, in order that the importance of their results may become more impressive on the minds of those readers who may wish to avail themselves of the advantages which these discoveries present in the construction of Galvanic batteries.

77. The electrical energies developed even by small plates of *cast* and *rolled* zinc are sufficient for the exhibition of most electro-magnetic experiments. That which is described in the note, page 137, Fig. 5, Plate VI. can be performed with a pair of plates, each two inches square, and a solution of nitric or nitrous acid, provided the magnet be of considerable power. With a pair of plates, each six inches square, the experimenter may safely calculate on success.

78. When the rotating cylinders in Ampere's experiment are both made of *zinc*, the one part *cast* and the other *rolled*, the apparatus performs excellently. A circular trough of *cast zinc*, exactly of the same shape as the wooden trough described in the note, page 137, and about an inch deep, has the extremities of a bent copper or brass wire soldered to the opposite sides of the upper part of the inner rim, as represented by Fig. 7, Plate VI. Another bent wire, *i i*, is furnished with a descending point in the centre of the curved part: to each extremity of this wire is soldered a narrow slip of *thin rolled zinc*. The wire, *i i*, is to be supported by its pivot in a small conical hole on the top of the curved part of that which is soldered to the *cast* zinc trough. The arms of the bent wire, *i i*, are to be sufficiently long to permit the slips of zinc to descend into the trough, but not to touch its bottom, and adjusted to an equal distance from the inner and outer rims, so that they, and the wire to which they are attached, can revolve freely on the point of suspension. The *trough* and *slips* of zinc in this apparatus form a Galvanic pair, being connected by the two bent wires and pivot. If this apparatus be placed on a magnetic pole, like that in Fig. 5, and nitric or nitrous acid, diluted with about ten times its quantity of water, be poured into the *trough* till it reaches a little higher than the lower edges of the slips of zinc, the moveable wire will revolve round the magnetic pole with considerable rapidity; if placed on the other pole, the revolving wire will proceed in the opposite direction. If two be employed at the same time, one on each pole, the experiment is more striking and elegant, as the suspended wires will be seen to revolve in opposite directions at the same time. In experiments of this kind it is sometimes convenient to amalgamate the pivot and pivot-hole to insure metallic contact, but if they be made quite bright amalgamation will not be necessary.

79. A zinc trough, such as that already described, was cast and afterwards *polished* in a lathe; but, in consequence of the asperities being thus removed from its inner surface, it would not operate in combination with the slips of *rolled* zinc until

corroded by subjecting it for some time to the action of a strong acid solution. When this trough became slightly corroded it began to operate in the capacity of copper in the *standard battery*, and the slips of *rolled* zinc evinced some tendency to revolve round the magnetic pole. As the inner surface became *more asperous* by corrosion, the electro-magnetic phenomenon was displayed with greater promptitude; and the revolution became *complete* and *constant* when the *asperous influence* on the surface of the *cast* part had attained an ascendency sufficiently great (in the capacity of copper in the *standard battery*) over the slips of *rolled* zinc; and the electro-magnetic powers became more and more energetic in proportion as the *difference* in the *asperous condition* of the surfaces employed became more decidely determined. Hence the electrical powers of the *cast trough*, in the capacity of copper in the *standard battery*, become *exalted* by long use, since the asperous character of the surface will be improved by every repetition of the acid. On the other hand, the action of the acid on the surfaces of the slips of *rolled* zinc causes them, to a certain extent, to become asperous also; consequently their electrical energies, in the capacity of zinc in the *standard battery*, become *deteriorated* by long use. With a knowledge of these circumstances, the experimenter can, at any time, employ his zinc to the greatest advantage for the display of the *influence* of *asperities* in determining its electrical character by electro-magnetic experiments. The *rolled* part is best when *new*, but can at any time be improved by hammering, or made smooth by a fine file; the other part cannot be too rough on its surface, and is, therefore, best when cast in very coarse sand. In Ampere's apparatus, the vessel which holds the acid solution is made of copper, and a cylindrical *rim* of zinc, in place of the *slips* which I have described, is soldered to the revolving wire: the *slips*, however, answer quite as well.

80. When the circular vessel for holding the acid solution is made of glass, having a tube with a closed top, for its central part to receive the magnetic pole, it becomes a most elegant apparatus; by means of which the experiment is pleasingly varied, and, in a philosophical point of view, the phenomena displayed are more decidedly interesting than by any other mode of exhibition. Fig. 8, Plate VI. represents a vertical section of the apparatus complete. On the crown of the central tube, *e*, of the glass vessel, *o e o*, is a small cup or pivot-hole. A bent wire, having soldered to it a finely-pointed *ascending* and *descending* pivot, is suspended by the latter in the small cup on the crown of the vessel; and on the *ascending* point is suspended another wire of like form. To each depending extremity of the inner wire is soldered a thin slip of zinc, and to the extremities of the other wire are soldered similar slips of copper—both metals descending into the glass vessel, and reaching to the same distance from its bottom, and free to move past each other whilst revolving on the pivots. These slips of copper and zinc form the Galvanic combination, or rather two combinations; and when a solution of nitrous acid is poured into the glass vessel till the metals become

slightly immersed, the electric streams will flow from the copper slips, up the outside wire branches, to the pivot; thence they will descend by the innermost branches to the slips of zinc, and the circuits will be completed through the fluid medium. Place this apparatus on the pole of a magnet, and, notwithstanding the magnetic pole will then be enveloped with the glass tube, its powers will permeate the glass, and operate on the Galvanic wires, which will revolve as if no glass whatever intervened; but, in consequence of the electric currents *ascending* in one pair of branches and *descending* by the other, those wires will revolve round the magnetic pole in opposite directions, and present an exceedingly beautiful compound motion. When an apparatus of this kind is placed on each pole of a horse-shoe magnet, a most imposing display of electro-magnetic action is thus accomplished. When a horse-shoe magnet of a proper shape is not at hand, any of those experiments may be performed on the poles of bar-magnets of sufficient power.

81. It has been observed (Articles 37 and note, page 136) that, when zinc is amalgamated, its power of decomposing solutions of sulphuric acid becomes so far deteriorated that the action is scarcely perceptible; whilst, at the same time, it forms a more energetic Galvanic arrangement when combined with copper, or other metals, than if not so treated. Zinc, therefore, in this state becomes an exceedingly convenient metal for the exhibition of some of the most interesting experiments in Galvanism, whether we regard the phenomena displayed to be contemplated by the philosopher, or for elegance of exhibition in the lecture room. And *couronne des tasses* of Volta, and the *decomposing wires* of Sylvester, are amongst those which may be exhibited in the most satisfactory and elegant style.

82. The *couronne des tasses* consists of a series of small glasses, partly filled with an acid solution, and are generally arranged in a circle. Slips of copper and zinc are soldered end to end, and so arranged in the glass vessels that the *copper* extremity of the *first* is in the same glass with the *zinc* extremity of the *second*, the *copper* of the *second* with the *zinc* of the *third*, and so on in regular sequence; so that each glass in the circuit will receive a piece of *copper* and also a piece of *zinc*, which have no further connection with each other than by the fluid medium—the metallic communication being from one glass to another, as in Fig. 9, Plate VI. If one of the compound metallic arcs be removed, the circuit becomes interrupted, and the extreme glasses become the poles of this miniature Galvanic battery. Gas will now be liberated at the surfaces of the zinc parts only—an effect which would be produced whether the copper were present or not. When the circuit is completed, by introducing the other compound arc, gas will also be liberated at the surface of every piece of copper in the arrangement. A solution of muriatic acid is generally employed in this experiment; but in consequence of that acid being sometimes of a yellowish tinge, and also acting chemically on the zinc, the beauty of the experiment becomes partly

obscured. When the zinc is amalgamated, and a limpid solution of sulphuric acid employed, no such obscuration takes place; neither will any chemical action whatever be observed at any other time than when the circuit is complete, and even then at the copper surfaces only—showing, in an exceedingly beautiful and impressive manner, the influence of electric currents in the development of chemical action, which, by this mode of experimenting, can be created or annihilated with the velocity of lightning at the experimenter's pleasure.*

83. The experiment instituted by Mr. Sylvester operates upon the same principle as the one last described: this, however, is a *simple*, the former a *compound* arrangement. The apparatus is represented by Fig. 10, Plate VI. and consists of a glass jar containing very diluted muriatic acid. Through a cork placed in the neck of the jar two wires are inserted, the one a short straight wire of zinc, the other a long bent wire of copper; by turning the latter round, its upper end may be brought into contact with the zinc, or separated from it at pleasure. When they are separate, the zinc only is acted on; but, when brought into contact, gas will be liberated at the copper wire also. By this apparatus, the experiment is generally exhibited on a very small scale, and gas appears at the surface of both metals.

84. Fig. 11, Plate VI. represents a glass jar, which is to be nearly filled with a limpid solution of sulphuric acid. In the jar is placed, in a sloping position, a slip of amalgamated rolled zinc: no gas will be evolved. Hold a slip of copper also in the solution, and no gas will appear while the two metals are separate. Let the lower edge of the copper slip touch the amalgamated piece: a cloud of gas will immediately ascend from every part of the copper surface, but not a single bubble from the amalgamated zinc. If the zinc be formed into a disc, and laid flat in the bottom of the jar, a copper wire, held vertically and touching the centre of the zinc, will liberate gas at every part of it which is below the surface of the fluid; and as no gas will rise from the zinc disc, this variation of the experiment will have a very pleasing effect. When the copper wire is formed into a spiral, the decomposition is very strikingly displayed, and the gas will ascend in the centre of the fluid in the form of a pillar, increasing in density from the bottom upwards.

85. Another very imposing variation of this beautiful and interesting experiment is accomplished by employing a disc of copper, through the centre of which, and at right angles to its plain, is soldered a wire of the same metal. The disc is to be pierced in several places with a sharp instrument for the passage of gas from its lower surface; and the length of the lower part of the wire is to be less than half the altitude of the fluid, and, with the exception of the point, to be covered well with sealing-wax: the upper part of the wire serves to hold it by. When this disc and

* The reader will find in Jameson's *Edinburgh Philosophical Journal*, for October, 1828, a description of some very ingenious applications of a soft amalgam of zinc in the construction of Galvanic batteries, by Mr. Kemp.

wire are introduced to the fluid, and the lower extremity of the wire in contact with the amalgamated zinc in the bottom of the jar, gas will be liberated from every part of the copper disc, and will ascend through the holes, and completely obscure the upper part of the acid solution, whilst the lower part of the fluid will remain as tranquil and limpid as if no process were in operation in the vessel. It sometimes happens that a few bubbles of gas rise from the point of contact, but this can be prevented by amalgamating the point of the wire, and permitting it to rest in a globule of mercury. A flat spiral may substitute the copper disc. (See Fig. 12, Plate VI.)

86. Whatever opinions may have been entertained as regards the influence of metallic contact in Galvanic arrangements, I am persuaded that this influence operates no farther in promoting Galvanic energies than by the superior conductibility of metals over all other bodies, for those energies are brought into play whether the metals (copper and zinc in the *standard battery*) touch one another or not, provided there be any conducting medium between them ; and the combination will be *more* or *less* active as the connecting material is *more* or *less* of a conducting character. Hence, if the copper and zinc be connected by a *fluid conductor*, Galvanic action will be produced, but not to that *extent* as if a *metallic wire* were the conducting medium.

87. Fig. 14, Plate VI. represents two wires in the same vertical plane, and parallel to a magnetic needle placed directly between them : the wires are furnished with cups at their amalgamated extremities for the purpose of holding mercury. Let the cup, *c*, of the upper wire be connected with the *copper*, and the cup, *z*, of the lower wire with the *zinc* in the *standard battery*: no circulation of the electric fluid can possibly take place in those wires while their other extremities are unconnected, consequently the needle will not be moved out of its natural direction; but if a copper or other metallic wire connect the two cups, *i i*, the circuit becomes completed, as by one continued wire once round the compass box from the copper to the zinc, and the needle will be deflected with its *north end* towards the *west* if the cups, *i i*, be at the *north side* of the apparatus, but towards the *east* if those cups be at the *south side.* If the *copper* were connected with the under wire and the *zinc* with the upper, the deflections of the needle would be in the reverse order.

88. Fig. 14, is the simplest possible form of the apparatus. When the wires are each passed several times round the compass box, still observing the same principle, the apparatus becomes much more efficacious in determining the character of feeble Galvanic energies. This apparatus may very aptly be termed the *unclosed Galvanometer*, in contra-distinction to the other, which has no opening in the wire.

89. If a piece of well burned charcoal *close* the Galvanometer, by connecting the cups, *i i*, the needle will be deflected ; but, because of an inferiority in the conducting powers of the charcoal, the angle of deflection will not be so great as when the instrument is *closed* by metal.

90. Let two wires of platinum have one extremity of each in a small glass vessel, but not in contact with each other, and their other extremities one in each of the cups, *i i*, of the *unclosed Galvanometer*—the battery being connected with the other cups, *c* and *z*. If now nitric acid be poured into the glass vessel till it reaches the platinum wires, the Galvanometer will become *closed* by the acid, and the needle will be deflected in the same direction as if the instrument were *closed* by charcoal or a metallic wire.

91. If, instead of wires, broad strips of platinum foil be placed in connection with the fluid conductor, the angle of deflection will be *much greater*; and the needle will be deflected *still more* by exposing *large platinum surfaces* to the acid in the glass vessel. The angle will also vary according to the *distance* between the pieces of platinum, becoming *greater* as they *approach* each other. By experimenting in this way it is easily discovered that electro-magnetic energies displayed will depend upon the *length* and *diameter* of the fluid conductor, as well as upon its natural conducting character. Attention must also be paid to the *copper* and *zinc* forming the Galvanic pair, for if the distance between them should *vary* during an experiment much error may result from that circumstance, as their Galvanic energies vary considerably by a difference in the distance of separation, being always *greatest* when the metals are *nearest* to each other in the acid solution, provided they do not touch.

92. The *unclosed Galvanometer* offers great facilities in experiments which are intended for comparing the conducting powers of various substances, and also of comparing the same materials under different *forms* and *dimension*. By this instrument we have an opportunity of ascertaining the relative effects on the needle of any two Voltaic pairs by directing their currents in opposite directions, an advantage peculiar to this Galvanometer.

W. S.

Artillery Place, *Woolwich*, *July*, 1830.

ON THE THEORY OF ELECTRO-CHEMISTRY APPLIED TO THE DISSOLUTION OF SIMPLE METALS IN FLUIDS.

SIXTH MEMOIR.

PART I.

1. Although Electro-Chemistry is usually dated from the experiments of Messrs. Nicholson and Carlisle, in April, 1800, the experiments of Fabroni claim a much earlier place* in the history of the science. The discoveries of Cruickshank and Davy, however, were those which tended most to lay the foundation of Electro-Chemistry; according to the laws of which, each individual body in nature is possessed of an innate electric force, which gives it a tendency to combine with other bodies, and form compounds of a variety of forms. But when other electric forces, superior to those which hold the constituents of any compound together are properly applied, decomposition takes place, and the liberated constituents arrange themselves in separate groups around the respective electric poles which effected their separation, each group proceeding to that pole from which it differs most in electric character or tension. The splendid discoveries of Davy effected a new epoch in chemical science, which placed chemical changes to the account of electric forces; but, in all cases of electro-chemical discovery up to the year 1830, the implements employed were Voltaic batteries of some form or other, in which three elements, at least, seemed indispensable: in most cases two dissimilar metals and a liquid were employed, and in some others two liquids and one metal; but in no instance had philosophers attempted to show that the dissolution of simple metals in fluid menstrua was due to electric forces. The dissolution of alloys had been suspected to be effected upon the principles of Electro-Chemistry, but the affinities of the old school could not be dispensed with in those cases where two distinct kinds of metal were manifestly not present.

2. In July, 1830, I published the first part of my "*Experimental Researches in Electro-Magnetism, Galvanism, &c.*" a portion of which is embodied in the fifth Memoir; but the electro-chemical researches which appeared in that work have been set apart for this place.

3. The beautiful and interesting experiment described in page 153 appears to be exceedingly well calculated to direct our ideas to the very fountain of chemical action;

* Nicholson's Quarto Journal, vol. iv.

and is, without exception, the most striking manifestation on record of the influence of simple Galvanic arrangements in starting into active play those potent energies which are exercised in the chemical change of matter.

4. Whilst the metals are unconnected, the very powers which are capable of reducing their symmetrical forms into rude heterogeneous masses are suspended within the boundaries of their own surfaces, or securely lodged in the fluid moleculæ which surround them; but, if once the metals touch, the magic spell is broken—tranquillity vanishes with the velocity of lightning, and the once dormant powers of Electricity, as if by enchantment, instantly spring into uncontrolled activity—flow with immeasurable celerity and precision of direction through the solid and fluid group, giving new *forms* and *positions* to their obedient elements. Again separate the metals—again the seal is closed—the electric spring has lost its powers, and receded to within the barriers of the inactive elements, where it once more reposes in concealment. Tranquillity is thus restored, and Chemistry ceases to exist.

5. If we could trace the analogy to more complex electro-chemical actions, some of the experiments already described in the fifth Memoir, and others which will soon be spoken of, will offer considerable facilities to such an inquiry. The electrical characters of metallic bodies become so very materially modified even by imperceptible transitions in the forms of their moleculæ, that it is next to impossible to select two specimens of the same mass which are precisely in the same electrical state; and kindred pairs of some of the metals, such as iron, zinc, tin, &c. form very active Galvanic combinations, the energies of which require no very nice Galvanometer nor experimental dexterity for their detection.

6. The difference, which by experiment is thus discovered in the electrical characters of considerable masses, is easily detected, by a similar process, in the smallest tangible fragments; and, by carrying our ideas a little farther, we readily discover that similar electrical relations may possibly exist in the very elementary metallic particles of which those fragments are composed. Hence it appears that even the smallest atoms of the same metallic body may *relatively* be in different states of Electricity, although the mass itself, taken as a whole, displays a decided and peculiar electric character in reference to all other metallic bodies.

7. With this view of the electrical state of metallic surfaces, it requires no superlative degree of penetration to discover that multitudes of Galvanic circles must necessarily spring into active play whenever a metallic body is plunged into a fluid medium suitable to the promotion of their energies; for if we consider the two wires in the experiment (Fig. 11, Plate VI.) as a simple elementary pair of particles already in Galvanic action, we can readily transfer our ideas to two particles in similar Galvanic operation on the surface of a piece of zinc, iron, tin, &c.; but as that surface, when extensive, consists of myriads of particles, all of which are relatively in different states

of Electricity, and in metallic contact with each other, there must also exist multitudes of elementary pairs in active operation, combining with oxygen, and liberating gas at their respective poles as decidedly as in any Galvanic arrangement whatever; and their multitudinous character alone prevents our distinguishing them separately.

8. It will not be necessary, however, to extend our ideas so far as to consider that every ultimate particle absolutely constitutes a Galvanic pole. The imagination will be considerably relieved, and it will also be more consistent with reason and observation to contemplate those particle in congregated groups—the relative electrical characters of which alone will satisfy every inquiry as regards the decomposition of fluid menstrua by metallic bodies.

9. When a metallic body is plunged into an acid solution, the points on its surface from which gas is liberated are by no means so numerous as we are frequently led to imagine; and, when the proportional quantity of acid to water is very small, those points may be easily distinguished from each other, and even enumerated by close attention.

10. *Experiment A.*—Let a glass tumbler be nearly filled with water, mixed with a few drops of sulphuric acid; introduce a piece of clean zinc, about the size of a half-crown, and let it rest at the bottom of the vessel. When this apparatus is perfectly steady, it will be observed, by looking attentively through the glass, that the points from which the hydrogen is liberated are comparatively but very few in number. The experiment, when the acid is properly diluted, is exceedingly interesting, and has a very pleasing effect. The gas rises in distinct streams, which do not mix into a confused cloud until they have arrived at a considerable height above the metallic surface. These gaseous fountains may be regarded as so many Galvanic poles, which will be more or less active according to the relative electric states of the different groups or parts of the metallic surface; and also according to the relative electric states of the constituent parts of the fluid medium. The *number*, as well as the *energies* of those poles or fountains, will likewise be regulated by these electrical qualities of the solid and fluid parts.

11. It very often happens that the fountains of gas are at some considerable distance from each other on the surface of the zinc plate; the observer may then select any one of them for a second experiment, which, in a philosophical point of view, is quite as interesting as the former, and proves most decidedly that those fountains are of Galvanic origin; for their energies may be so completely controlled that the experimenter may exalt, diminish, or even annihilate them at pleasure, upon the most rigid Galvanic principles.

12. *Experiment B.*—When an individual fountain has been selected, let a thin copper wire touch the zinc plate at a point not very distant from it: gas will not flow from the fountain so rapidly as at first, but will be copiously liberated at the surface of the copper

wire. Bring the wire still nearer to the fountain, and the stream of gas will be much thinner, and it will become more and more slender as the wire approaches it; till at last, when the wire has arrived sufficiently near, the fountain will cease to play. If the wire be made to recede gradually, the fountain will re-commence with the appearance of an exceedingly thin, slender stream, the density and dimensions of which will increase as the wire becomes more distant; and when the wire is entirely removed from the zinc plate, the energies of the fountain will appear as active as at first.

13. *Experiment C.*—When several copper wires touch the zinc plate in different places, each wire will liberate gas, and the activity of the fountains on the surface of the zinc will be considerably abated. When the wires are sufficiently numerous, every one of the fountains will be extinguished, and gas will be liberated at the copper wires only—showing, in a very impressive manner, that the *natural Galvanic energies* of the zinc which produce those fountains are susceptible of being overpowered by the *artificial Galvanic energies* which have superseded them, and which have produced new fountains of gas at the newly-established *artificial poles.*

14. This variation of the experiment will give the observer a pretty good idea of what may be very appropriately termed *natural Galvanic circles;* proving also that multitudes of them may exist, in active play at the same moment, on the same metallic surface.

15. *Experiment D.*—The illustration will be considerably improved if the zinc plate be amalgamated and a few coarse copper filings scattered on its surface. It will be observed that every particle of copper will become a Galvanic pole and liberate gas with the greatest promptitude, and highly imitative of the *natural Galvanic process* on the surface of a piece of zinc when plunged into a similar solution.

16. *Experiment E.*—If, in place of the copper wire (Experiment B), a narrow slip of amalgamated zinc be employed, the fountain, instead of being annihilated, will become more active by a close approximation of the amalgamated piece, because the latter has an opposite electrical relation to the zinc to that displayed by the copper. These appearances are very easily produced when the fountains are but few in number and not very active; but it frequently happens, when a slip of amalgamated zinc is employed, that part of the mercury runs to the other piece and spreads itself over the fountain, which immediately becomes extinct, by having its electrical character changed.

17. To prevent confusion, it will be necessary to draw a line of demarcation between Galvanic *combinations* and *circles* of the two different characters—*natural* and *artificial. Natural Galvanic combinations* are those which I consider to exist naturally in the same piece of metal, whether it be an *amalgam*, an *alloy*, or a *pure metal*—the latter class of which, I imagine, are but very few in number. These *natural combinations* are brought into play when the piece is plunged into a fluid medium suitably adapted

to promote their energies; as, for example, zinc into diluted sulphuric acid. When thus situated, *natural Galvanic circles* are formed, the energies of which I consider to be the cause of what is termed the *chemical action* between the metal and the acid solution, and consequently *that action* a *Galvanic phenomenon.*

18. *Arificial Galvanic combinations* are all those which are formed of two or more pieces, whether those pieces be of two or more distinct metals, or of the same kind of metal in different states of compression, figure, polish, size, &c., or of any combination whatever consisting of more than *one piece of metal* and *one fluid medium.* Hence the copper wires or filings, with the zinc plate in the preceding experiments, formed *artificial Galvanic combinations*, and were merely illustrative of the *natural Galvanic combinations* which are considered to exist only in the self-same piece.

19. Now, it is evident by the illustrative Experiment D, that all the natural combinations on the surface of a piece of metal may be contemplated collectively, and regarded as one combination only; for the amalgamated piece of zinc, giving off no gas, can have no very energetic natural combinations, and those which do exist are too insignificant to compete with the artificial combination formed by the plate and one single particle of copper only. These natural combinations may, therefore, be considered as absolutely *neutral* amongst themselves when several particles of copper are in contact with the amalgamated plate, the surface of which may be then regarded as in *one uniform state of Electricity*, and the particles of copper as uniformly in the opposite state. But each particle of copper liberates gas, and is consequently a Galvanic pole, and necessarily belongs to a Galvanic circle which possesses a pole of the opposite character. This latter pole is to be found in the amalgamated zinc; and to the same source we are to trace as many Galvanic circles as there are particles of copper in contact with it. But the amalgamated zinc is in *one uniform state of Electricity*, and, therefore, notwithstanding the *number* and *individuality* of poles which it supplies, this uniformity of its electrical character pronounces it to be *one individual*, *aggregate*, or general pole to all the existing Galvanic circles. Precisely the same reasoning holds good by reversing the experiment; for if several particles of zinc be placed on a copper plate, each particle will become a Galvanic pole, and the copper, by embracing all the opposite poles in one *uniform electric character*, will become *one aggregate* or general pole to as many Galvanic circles as there are particles of zinc in contact with it. Hence we discover that a Galvanic combination may be divided into as many branches as we please: it may have one *positive* pole and one *negative* pole, it may have one of either character and as many of the other as we please, or it may have a multiplicity of both at the same time, and still be *one* Galvanic combination only. It appears, therefore, that much of the difficulty in comprehending the multitudinous character of natural Galvanic combinations speedily vanishes by this mode of reasoning; for whether we contemplate those combinations *separately* or *collectively*,

whether as an *assemblage* of combinations or as *one* combination only, we uniformly arrive at the same conclusion, viz., that a number of *points* on the surface of a piece of metal, although *relatively* in different states of Electricity, are, when taken *collectively* and as a whole, uniformly in the *same state* as regards all the rest of the surface, which must of necessity be as uniformly in the opposite electric state. Those points will, therefore, become distinct Galvanic poles to circles which may be traced to one *aggregate* or *concentrated* pole of the opposite kind.

20. Now, as those points or poles which have hitherto been considered separately and distinct from each other, are all of the same electrical character (say negative) as regards the aggregate positive pole, they may also be regarded as forming one aggregate *negative pole*, and consequently the whole of the metallic surface in *two distinct states* of Electricity. By this simplification we find that, notwithstanding the multitudes of natural Galvanic combinations which may possibly exist on the surface of a piece of metal, and the various changes of energy and position which they may experience during the natural Galvanic process, they will always be divided into two *grand divisions*, which will be *positive* and *negative* as regards each other; and the metallic surface itself will at all times operate as *one natural Galvanic combination only.*

21. The reader will now find little difficulty in comprehending what I have considered as *natural Galvanic combinations.* They are supposed to exist in all the metals, and perhaps in other bodies, but with different degrees of energy in each. These views of *natural Galvanic combinations* appear to me well calculated to remove some of those difficulties which have hitherto been stumbling blocks in this branch of philosophy, but what reception they may experience from others is not for me to determine.

22. Considering that *heat* might possibly develop the energies of natural metallic combinations, I was induced to make experiments with single pieces of metal, and have found that some of them become highly *electro-magnetic* by that process. The experiments are described in the second Memoir.

23. Whatever may be the cause which gives a different electric character to dissimilar metals, it is reasonable to expect that the same influence will be extended to the particles of other dissimilar matter, and that the simple gases have each a peculiar electric character; and that when combined, either in simple pairs or in any other proportions, the compound particles will necessarily be as distinctly electro-polar as the copper and zinc in a Voltaic pair,* and as susceptible of polar separation by

* The Voltaic plates, which are usually manufactured in London, and sold in the instrument makers' shops, are a pair of discs, one of copper, the other zinc, which vary in size from one to ten or more inches in diameter, according to the fancy of customers. One surface of each disc is made perfectly flat and smooth; the other side of each is furnished with a socket, in which is cemented a glass handle, for the purpose of insulating the plates from the hands of the operator. When these plates are held by the glass handles, and their smooth faces brought into close contact, a portion of the electric fluid which previously occupied the copper passes to the zinc plate, which is easily detected by separating the plates suddenly, and applying either of them to the cap of a gold-leaf electroscope.

superior electric forces as the particles of red lead and sulphur in the long neglected but highly interesting experiments of Lichtenburg and Cavallo.* Hence it is, and from no other known cause, that chemical compounds are decomposable and their constituent elements uniformly arranged by electric action—some requiring a greater and some a lesser degree of force to effect their separation.

24. The electro-polar forces naturally existing in some of the metals, even in the purest state they have hitherto been procured, are perhaps more energetic than those of any single Voltaic pair that can be formed of two distinct pieces of dissimilar metals; and some of the most formidable of those forces (on the surface of metals in common use) are presented by iron and zinc.

25. If a piece of zinc or iron be immersed in distilled water, its polar particles and those of the water being brought to within the sphere of each other's action, begin to operate on each other; and the reciprocal polar forces mutually assisting each other are enabled to separate the constituent particles of the aqueous compound: the oxygen being carried to the *positive* poles of the metallic surface, and the hydrogen to the *negative* poles, according to the well-known laws of electro-chemical action by the energies of the Voltaic pile. By this means a continual interchange of the electric fluid takes place between the polar points on the surface of the metal and the fluid particles to which they are presented; and multitudes of elementary electric currents are thus produced, which flow through the metallic and fluid media in directions as exact as in any Voltaic pair, and those directions as diversified as the variety of positions of the elementary poles in their general distribution over the surface of the metal.

26. The positions of the polar points on the surface will frequently change during the dissolution of the metal on various accounts, depending upon the deposition of oxide formed by the first and subsequent currents, the texture of the metallic points, their polish as they are arrived at during the process, their crystalline structure, and perhaps upon many other causes of a still more recondite nature. Every succeeding surface of the metal will thus present its peculiar arrangement of assailing poles to the contiguous fluid particles, which, in consequence of their electric omniparity, susceptibility and independency of motion, offer neither inequality of force nor united energy to resist the attack, but obsequiously arrange themselves under the influence of their assailants—become vanquished individually at every point, and resign their constituent elements to the superior electric forces to which they are continually exposed.

* If the powders of red lead and sulphur be intimately mixed together, and put into a spring puff, such as hair-dressers sometimes use for powdering wigs, and then projected from the puff through the air, against a resinous cake or paper tea-board, the different parts of whose surface is in different electric conditions—or, if you please, some parts positively and others negatively electric—the particles of the sulphur will fly to the positive surfaces, and those of the red lead to the negative surfaces, showing that the red lead and sulphur were in different electric conditions. Many other mixtures of powders show a similar phenomenon.

27. It is true that the decomposition of distilled water is slow by the natural polar energies on the surface of a piece of zinc or of a piece of iron, but still the process is considerably more rapid than that which would proceed from the electric action of any binary metallic combination, on whose surfaces, whilst separate, the natural electro-polar forces are not energetically displayed. But if the polar energies be of an exalted character on zinc and on iron, they are infinitely more so on the surfaces of sodium and potassium, metals which decompose water with an amazing rapidity.

28. It will now be easily understood how it happens that a piece of zinc (and the same reasoning applies to iron and some other metals), which before it entered the water was quite smooth and clean, becomes rough and asperous by long immersion; for the oxygen, being the only part of the water which unites with the metal, becomes unequably distributed over its surface—at its positive poles only: the negative poles of the metallic surface having nothing to combine with are left unmolested, and for a while remain as prominent as at first. But, as the process advances, the changes of position of the metallic poles, which are continually going on, being attended by corresponding changes in their electric conditions, those points on the surface which previously had been electro-negative will occasionally become electro-positive, and consequently will, in *their* turn, become blended with oxygen liberated from the aqueous particles to which they are immediately presented: hence it is that every part of the surface becomes eventually oxidized. But at whatever period of the process we examine the metal, its surface is found to be asperous, which is a natural consequence of the multitude of electric poles which had been in play in the electro-chemical process.

29. I have selected distilled water for the fluid medium merely to simplify the illustration of the view which I have taken of the cause of this class of chemical phenomena; but the same principles equally apply to the dissolution of metals in other fluid menstrua, and are much more beautifully displayed when the action is of a more vigorous character. If, for instance, the water were to be mixed with an acid (say the sulphuric), the polar energies would become considerably more active, and the decomposition of the liquid and disappearance of the metal proportionally more rapid: the surface of the latter would soon become exceedingly rough, but the asperities would frequently change their positions before the metal entirely disappeared. Moreover, a rough zinc surface promotes this Voltaic action to a greater extent than one which is smooth; hence it is that the action is not so formidable whilst the zinc surface is smooth and clean at the first immersion, as it is a short time afterwards when the surface has assumed a more decided asperous character. And hence also that cast zinc has more formidable poles than rolled zinc, not only at first but during the whole time of the metal's dissolution, and consequently becomes more rapidly destroyed in similar acid solutions.

30. It is also on this account that *cast* zinc loses a greater portion of its real electric character, as a distinct metal in the Voltaic series, than rolled zinc does, and forms with copper a feebler combination than when in the latter compressed state; and when the natural electric poles on a tolerably smooth surface become nearly extinct by a covering of mercury, the zinc assumes its real electric character more decidedly than in either of its former conditions; on which account it forms, with copper, a still more formidable Voltaic combination than even pure zinc rolled does, without such electro-neutralizing amalgamation.

31. *Experiment F.*—If a slip of zinc, of any breadth whatever, bent into the form, *a o b*, Fig. 16, Plate VI. and have its extremities, *a* and *b*, immersed to the same depth in an acid solution, a compass needle, *n s*, placed as in the figure, will soon indicate the presence of an electric current in the parts between which it is placed; and, by its various movements and occasional stationary positions, we learn that electric currents set in, sometimes in one direction and sometimes in the other, during the dissolution of the metal in the fluid menstrua, which is a consequence of the manifold changes of number, position, and energy of the natural electric poles on the surface of each immersed extremity of the metal.

32. *Experiment G.*—If the end, *a*, of another similarly formed slip of zinc be immersed in a strong nitric acid solution, and the end, *b*, in a feeble one, placed in two cells of a trough or box, having a bladder partition between them, the needle will soon be steadily deflected, indicating a current to be running along the metal *from a to b*—a phenomenon easily accounted for upon the principles I have been endeavouring to establish; for the stronger acid solution giving greater facility to the play of the electric currents proceeding from the polar energies on the surface of the metal immersed in it, than the feeble acid solution does to the play of electric currents on the other surface, much of the real electric character of the extremity, *a*, will be lost; or, if you please, it will receive *more* fluid from the stronger acid solution than is received by the other extremity from the feebler; on which account it will be in precisely the same electric relation with the other extremity as copper is with zinc, and will consequently operate as a distinct metal in combination with the extremity *b*, which has not its natural electric character so much disturbed. A slip of copper exhibits facts of a similar nature.

33. I know of no received theory of chemical action that will satisfactorily explain the *corrosion* of metallic surfaces, when exposed to acid and other fluids; nor why some metals are dissoluble by almost any liquid to which they are exposed, whilst others require certain acid or alkaline fluids to accomplish their complete dissolution. These problems, which have so long been shrouded in mystery, may now probably receive a natural and easy solution. The surfaces of the purest metals are unequably electrical, and consequently electro-polar; but their natural combinations are less

powerful than those on the surface of metals of a coarser texture. Hence it is that acid particles, susceptible of separation by feeble electric forces, become necessary to bring their polar arrangements into electro-dynamic play: this once accomplished, their disfiguration is certain, and the multitudes of electric whirlpools to which they are exposed eventually terminate their solid existence.

34. The same principles which explain the dissolution of metals in acid and other solutions apply equally to the precipitation of metals from those liquids of which they form a part. A piece of iron, immersed in a solution of sulphate of copper, will presently have its natural electro-negative poles capped with metallic copper, which are so exceedingly numerous that the surface becomes nearly cased in a very short time. The positive poles, in the meantime, are supplied with oxygen from the liquid, which unites with the iron, and a new compound is formed. The copper now attached to the iron, and the uncovered parts of the latter metal, form new Voltaic combinations, and the deposition goes on whilst there is a sufficient degree of electric energy to withdraw the cuprous particles from the liquid in which they are suspended.

35. The "Lead Tree,"* the *Arbor Dianæ*, and other metallic arborizations, are phenomena of the same class, and are susceptible of a similar explanation; so are the amalgamations of metallic surfaces by the interference of acid and other solutions. Heat very often facilitates the process, and is sometimes indispensable to liquify the covering metal. Such is the case in the various processes of tinning iron and other metals, either in sheets, by dipping or otherwise, and in the process of soldering. The implement called a soldering iron, when soft solder is used, is absolutely a lump of copper. When this is heated just to below redness, it becomes coated with solder much more readily by the intervention of muriatic acid than by anything else; and iron or zinc, to which the tinmen usually apply a solution of muriate of ammonia, is tinned with much more facility by covering the surface with dilute muriatic acid.

36. The different degrees of facility with which metals and oxygen combine with each other is a problem in chemical science which by no means finds an intelligible

* The "Lead Tree," or "Philosophical Tree," as it is sometimes called, is thus made:—Dissolve an ounce of acetate of lead in about half a pint of water. When the sediment has settled, pour the clear liquor into a large vial, and suspend in it a piece of clean zinc. The bottle is now to be placed where it will not be shaken, and, in a few hours, brilliant films or metallic leaves will be suspended from various parts of the zinc surface.

When a large metallic arborization is wanted, a plain globular decanter will give it the best appearance. The solution, in proper proportions may nearly fill the decanter; and a stout piece of either rolled or cast zinc, suspended in the axis of the vessel, as before stated.

If a piece of zinc and another of copper be united end to end by solder, and bent into the form of the letter *u*, and suspended by the bend in a solution of acetate of lead, these two metals and the liquid form an artificial Voltaic circle; and lead will be re-metalized in beautiful crystal on the surface of the copper, but not in such abundance as at the natural negative poles on the surface of the zinc.

The Arbor Dianæ.—"If a little mercury be poured into a bottle nearly filled with solution of nitrate of silver, and the bottle be left for some time undisturbed, the silver is precipitated in a beautiful form, resembling the branches of a tree, which is the *Arbor Dianæ*."—*Henry's Chemistry.*

solution in the doctrines usually applied. The production of the "lead tree," already mentioned, may again be referred to as illustrative of this fact; and of the imperfection of every theory of the formation of that beautiful product which regards not the electro-polarity of the zinc, and the influence of electric currents as the most essential of its operative principles. The latter of these principles has long ago been admitted to perform an active part in every period of the process subsequently to the first attachment of the re-metalized lead to the surface of the zinc. But where are we to look for a satisfactory explanation of the cause which liberates and leaves suspended the *first* particles of the lead? The "superior affinity of oxygen for zinc" has long been a subterfuge, rather than a satisfactory solution of this curious problem. The doctrine of affinity would at once shroud the zinc surface with oxygen, to the entire exclusion of the revived lead, which, having no access, could have no opportunity to cling to the metallic zinc, but would fall, by its superior gravity, to the lower parts of the fluid. If it be admitted that the first revived particles would, for a time, remain suspended, still the subsequent part of the process would remain enveloped in obscurity, because of the want of metallic contact between the zinc and the liberated lead; and to admit a sufficiency of conduction in the oxide would be an unnecessary and unscientific concession, whilst more familiar principles are at our command, and an easy and natural explanation placed conspicuously before our eyes.

ON THE THEORY OF ELECTRO-CHEMISTRY APPLIED TO THE DISSOLUTION OF SIMPLE METALS IN FLUIDS.

SIXTH MEMOIR.

PART II.

On a Peculiar Class of Voltaic Phenomena.

(Read at the Glasgow Meeting of the Britisth Association for the Promotion of Science, 1840.)

37. The great interest excited by the development of those facts which show the various means by which modifications of chemical action on metallic bodies by acid menstrua are produced, having induced the British Association to grant pecuniary aid to Professor Schöenbein, to enable him to pursue those experimental inquiries in which he had previously been so successfully engaged on this subject, it cannot be presuming too much to suppose that the Association would be desirous of becoming acquainted with every fact connected with so interesting a branch of physical science. And as I think it possible that Professor Schöenbein may not be in possession of some particular facts of this class which were discovered several years ago, and as the theoretical views which accompanied the publication of those facts appear to me to be

applicable to every phenomenon of this peculiar class hitherto made public, I now venture to introduce them to the notice of the British Association, simply as adscititious data, independently of which any historical view of the discoveries which have been made in this class of phenomena, and the theoretical notions which have been advanced for their explanation, must necessarily be incomplete.

38. The experiments to which I allude were published in the year 1830, in a pamphlet, a copy of which accompanies this paper; and, as the experiments are clearly described, and the theoretical views which I then entertained respecting them are unequivocally stated in the original, I cannot do better in this place than to refer to them as they stand in that work.*

39. Having thus directed the attention of the Association to the character of the phenomena which I had discovered and published more than ten years ago,† it will appear obvious, by comparing them with the extensive series of facts which have subsequently been developed by various experimenters, that the whole are belonging to one and the same class of phenomena; that they have an electric origin, and are particular cases of, and easily traceable to, the same general laws of electro-chemical action which I have so clearly portrayed in the pamphlet already referred to.

40. We must not, however, overlook the labours of Bergman and Keir, the latter of whom has given a long list of discoveries of this class of facts,‡ all of which are as highly important as any that have been subsequently developed. Mr. Keir clearly describes some of those facts which have appeared as novelties within the last few years, and has shown that iron acquires that *altered*|| state by the action of nitric acid, which Sir John Herschel met with in his experiments, and has called *prepared*§ state, and that Schöenbein and others call the *peculiar* or the *inactive* state.

41. The fact also that iron in certain conditions will not precipitate copper from its sulphate and other solutions, as recently observed by M. Schöenbein, was one of the many beautiful phenomena discovered by Keir, and clearly described in the *Philosophical Transactions*, for 1790. Keir describes a number of similar experiments on the solutions of various metallic salts, and the phenomena in every case are obviously of the same class, and easily shown to be of electrical origin.

42. I have looked very attentively to the experiments of Herschel, Schöenbein, Andrews, Noad, &c., and I have repeated and varied many of them, and instituted

* A copy of the work was sent to the Secretary.

† Sir J. Herschel's paper on this subject, which is next on record, is dated August 19, 1833. See Annals de Chimie, or Philosophical Magazine, for October, 1837.

‡ Philosophical Transactions of the Royal Society of London, 1790, page 353; also Hutton's Abridgement of ditto, vol. xvi. page 694; and Annals of Electricity, vol. v. page 427.

|| Kier calls that iron which is active in nitric acid, "fresh iron;" and that which has become inactive, "altered iron."—See Annals of Electricity, vol. v. page 439.

§ Philosophical Magazine, for October, 1837, page 330.

others to a considerable extent, in order that I might be enabled to ascertain how far those theoretical laws which I had set forth would become applicable to the phenomena by the severest test which they could present. The electro-chemical phenomena developed by the action of two distinct metals, or by the action of two pieces of the same kind of metal, in acid or in alkaline solution, are so easily traced to Voltaic Electricity that no difficulty whatever is presented to their explanation. Sir Humphry Davy, in his Bakerian Lecture for 1826, has produced several interesting facts of this class with one kind of metal only; and several others are described in my pamphlet already alluded to, and are similar to some of those which Professor Schöenbein has met with and inquired for an explanation.*

43. It may probably appear singular that I should compare Keir's experiments on iron, in which only one metal is employed, with another in which two are made use of. I therefore wish it to be understood, that on the surfaces of those simple metals which are *active* in any acid solution, the particles are as decidedly in different electric conditions as any *two* distinct metals can possibly be, and act Voltaically on the liquid accordingly. Hence, in an electrical point of view, each simple metallic body presents different electric surfaces, which are equivalent to a Voltaic combination of two or more distinct metallic bodies. This once admitted, the *inactivity* of a metal, after some minutes immersion in an acid solution, follows as a matter of course, for the very same reason that two distinct metals become inactive after being some time in an active Voltaic condition. Their *relative* electric conditions have suffered a change, and they become inactive in that particular liquid, though they would still be active in another; and a new pair of metals, or a new piece of a *single metal*, would also be active in the old liquid.

44. There are several experiments described, both in the Bakerian Lecture for 1826, and in my pamphlet, in which the metals employed have changed their electrical characters, and the currents change their directions accordingly. Iron is very susceptible of these electrical mutations. A combination of two pieces of iron, as nearly alike as they can possibly be made to appearance, will sometimes exhibit several mutations of this kind before they finally cease to produce a current. In their first condition they are active, and A is positive to B; in a short time they are neutral to each other; afterwards B becomes positive to A, and becomes more and more so till the needle indicates the maximum of action. The action again declines, and again they pass through the neutral point, and again A becomes positive to B; and, after several mutations of this kind, the needle indicates that the two pieces are so nearly alike in their electrical characters that they are unable to produce an appreciable current. But *agitation* in the old liquid, or *immersion* in a new one, again brings them into a state of electrical activity. Similar electro mutations, and final neutrality,

* Philosophical Magazine, for February, 1837, p. 133-4-5.

take place on the surfaces of single pieces of metal; and there can be no doubt but this was the case both in Bergman and Keir's experiments, as well as in those of more modern date.

45. There are different causes for metallic bodies changing their electrical characters by Voltaic action, all of which will be found in the attachment of one or more of the constituents of the liquid employed. Hence the attachment of oxygen, nitrogen, metallic particles, &c. to metallic surfaces will change the electrical character of them, and in many cases completely neutralize them as regards particular liquids, though they may still be sufficiently active in other liquid bodies. Hence the Voltaic energies on the surfaces of gold and platinum are too feeble to decompose any of the simple acid solutions, though sufficiently powerful to become active in their well-known solvent. The particles of the liquids, and of their constituents, have also their electrical influence in all Voltaic actions, and are electrically modified as decidedly as the surfaces of solid metallic bodies.

46. In all cases where simple metals are first active and afterwards inactive in acid solutions, it should be borne in mind that before they become inactive they are absolutely in *metallic solutions*, by the dissolution of a portion of their own substances. Hence, in those experiments by Bergman and Kier, the cases were precisely alike, whether the iron was immersed in an already-prepared metallic solution, or immersed in nitric acid only. And the more modern experiments with bismuth, &c. are of precisely the same character; and their inactivity, produced by the re-attachment (precipitation) of their own previously-dissolved metallic particles, which give a new electrical character to the surface, and thus reduce the Voltaic energies too low to remain any longer active on that particular liquid, though still sufficiently active to operate on a different one. Hence the cause of their brightness during their *apparently* inactive state, for in reality no metal is perfectly inactive when a fluid is presented to it suitable for a display of its Voltaic energies.

47. With respect to other phenomena, in the display of which only one individual piece of metal is employed, as first shown by Kier, they remain without even an *attempt* at explanation by any of the philosophers under whose notice they have appeared. Sir John Herschel pronounces them as of an "extraordinary character;"* Professor Andrews, after giving some very satisfactory explanations of several phenomena, acknowledges that he "can offer no explanation of most of the particular facts which have been described;"† and Professor Schöenbein has not yet made public any explanation of them whatever.

48. Under these circumstances, it would be impossible to form any correct idea of the nature of the theoretical views on this subject which Professor Schöenbein will offer to the British Association at the Glasgow meeting. If, however, that philosopher

* Philosophical Magazine, for October, 1837, page 333.

† Ibid, for April, 1838, page 311.

should honour me with an attentive perusal of those facts and theoretical explanations to which I have already solicited the attention of the Association, it is possible, I think, that he will be enabled to find an easy and natural explanation to every phenomenon hitherto developed in this peculiar class, the whole of them being traceable to those laws which, though many years before the public, he may probably till now never have had an opportunity of becoming acquainted with.

49. I have shown that zinc, which is the most energetic metal in diluted sulphuric acid, may have its action either *partially* or *totally* annihilated in that liquid by the contact of copper wires (11); and I have shown that amalgamated zinc is almost neutral with solutions of sulphuric acid, but that it is still active in solutions of nitric acid (fifth Memoir, 37 and note). These appear to be cases in point, and the latter is of precisely the same character as those shown by the iron in Kier's experiments. In one liquid it is *inactive*, in another it is *active*.

50. The explanation in these, and in all similar cases, is to be found in the difference of Voltaic action in the solid and fluid bodies employed.

W. S.

Royal Victoria Gallery of Practical Science,
Manchester, June, 1840.

SUPPLEMENT TO THE SIXTH MEMOIR.

SUGGESTIONS ON THE ELECTRO-DECOMPOSITION OF MIXED METALLIC SOLUTIONS.*

In the electro-decomposition of simple metallic solutions, and perhaps in that of all other kinds, it appears to be an invariable law that the constituent carried to the *negative pole* is the best electric conductor (the metal) in the compound. This being an established fact, it is probable that the same law would be observed when more than one metal is held in the solution operated on. The best conductors in such solutions ought to be carried to the negative pole, in preference to any of the rest; or at least in an earlier part of the process; for instance, a solution of copper and zinc, in nitrous acid. The copper, being a better conductor than the zinc, ought, according to this law, to be revived at the negative pole in the earliest part of the process, and the zinc not until a later period. Upon the same principle, gold ought to be recovered before zinc from a solution holding them both; and, for the same reason, any metal suspected to be a compound ought to have its constituents separated by a similar process. The same law, if universal, ought to be observed in all metallic

* Philosophical Magazine, for November, 1833.

solutions whatever. If, therefore, those metals which are apparently the most pure and simple should still happen to be compounds, it is highly probable that, by attending to and operating upon this principle, their decomposition would be very much facilitated, if not entirely accomplished.

I some time ago made a few experiments on this point, but I have not yet had time to prosecute them far enough to obtain a sufficient number of accurate results to form just notions as to the probable extent to which this mode of analysis may be carried.

With regard to copper and zinc, the law appears to hold good when the battery is not too powerful. I have employed one hundred pairs of one-inch plates, and also one hundred of two-inch plates, and have obtained the same results with both batteries. The metals were held in solution by sulphuric acid and water. Whilst the battery was active, both the copper and zinc were deposited on the negative platinum wire; but when the power was less active, the copper alone was revived.

I throw out these hints in order that others, better circumstanced than I am, may if they please take advantage of them. The field, I believe, is quite new, and appears to me to be worthy of investigation.*

W. S.

Artillery Place, *Woolwich*, *Sept.* 23*rd*, 1833.

RESEARCHES ON VOLTAIC COMBINATIONS.—A NEW BATTERY OF CAST IRON AND AMALGAMATED ZINC.—A COMPARISON OF THE POWERS OF VARIOUS VOLTAIC BATTERIES.—ELECTRO-MAGNETIC TELEGRAPHS, &c.

SEVENTH MEMOIR.

1. About twelve years ago, I engaged in an extensive series of experimental inquiries respecting some of the principal conditions necessarily connected with the action of Voltaic batteries, during which I arrived at some remarkable results, which I then conceived might probably be advantageously applicable in the formation of that peculiar class of electrical apparatus. Some of these results I published in the year 1830, in a pamphlet, entitled "*Experimental Researches in Galvanism, Electro-Magnetism, &c.*"† Since the time of my pamphlet making its appearance, some of those results which I described in it have become available in the hands of other

* Since this hint appeared in the *Philosophical Magazine*, metals have been separated from their ores by the Voltaic batteries both in France and Russia.

† See fifth Memoir.

experimenters, and some others have come into general use in almost every form of Voltaic battery.* There are, however, other discoveries which I then made and intended for the second part of that pamphlet; and, as they have not yet been met with by others, nor in any way made public, only occasionally at my lectures—and as they appear to be of some importance, whether viewed as theoretical or practical data—I venture to give them a place in this Memoir.

2. In the pamphlet already alluded to, I have shown (see fifth Memoir, page 137) that when two similar pieces of iron are placed one in each of two strong solutions of nitric acid in water, of different degrees of strength, having a bladder partition between them, they formed an active Voltaic pair. A Galvanometer, with a heavy needle, four inches long, supported on a pivot, was employed in these experiments, "and the needle would frequently stand at an angle of 35°, particularly if the stronger portion of the acid solution was not very feeble; and these energies seemed to improve with an increase of acid in that portion of the fluid."

3. At page 138, paragraph 40, under the head "Iron and Nitrous Acid," I have shown that "the electric relations of the two pieces of polished iron, when placed in two portions of this acid, very differently diluted, or the one piece in the acid solution and the other in water, are precisely of the same character as when the *nitric* is employed; but the electric energies displayed are more energetic, &c."

4. From the facts discovered in these experiments, I was led to construct a compound battery of ten small pairs of iron plates, in wooden cells, each cell being furnished with a bladder partition. The iron which constituted what I have called "a pair," was, however, merely a single piece, or long strip, which, by being bent in the middle, was easily adapted to unite two troughs—one of its ends being immersed in the *strong* acid solution, and the other end in the *feeble* acid solution of the vicinal trough, and so on throughout the series. With this battery I could decompose water, ignite metals, charcoal, &c. to a certain extent, as decidedly as by any Voltaic battery whatever; but, as its chemical and calorific powers did not meet my expectation, I proceeded no farther with it. I discovered, however, that iron held a more elevated rank amongst the metals when associated with amalgamated zinc, in Voltaic series, than had ever been noticed by any other experimenter. Indeed, at that time, amalgamated zinc had never been employed in Voltaic batteries, except in a semi-liquid form, by Mr. Kemp, an ingenious chemist at Edinburgh. Sir Humphry Davy first noticed that amalgamated zinc acted better than pure zinc, when amalgamated with copper, in a single pair; but I believe that the employment of amalgamated rolled zinc originated with my own experiments, and I formed compound batteries of cylinders of zinc and copper, which worked exceedingly well with diluted sulphuric acid.

* Amalgamated rolled zinc.

5. I discovered also that cast iron and wrought iron performed very differently in Voltaic combinations with zinc, the cast iron forming the more energetic combination with that metal, especially when well amalgamated. I discovered, moreover, that amalgamated iron holds a higher rank than either cast iron or wrought iron, when Voltaically associated with zinc, and that, therefore, any transference of mercury that might occur from amalgamated zinc would rather be favourable to the action than otherwise—a circumstance so diametrically opposed to that which occurs with amalgamated copper, as to give a preference to iron over that metal in Voltaic associations with amalgamated zinc, especially when the excitation is carried on with diluted sulphuric acid. Lately I have been induced to construct larger batteries of cast iron and amalgamated zinc than I had ever before attempted, which, with their performances in the display of phenomena, I will now describe.

6. The first battery of this kind that I constructed since my appointment at this Institution* consists of ten cylindric jars of cast iron, each eight inches high and three-and-a-half inches diameter, with the same number of amalgamated zinc cylinders, of the same height as the iron ones, and about two inches diameter. Each pair of these metals is connected together by means of a curved, stout copper wire, one end of which being soldered to the iron and the other to the zinc, as shown in Fig. 1, Plate VII. The zinc of one pair is placed in the iron jar of the next, and so on throughout the series—contact being prevented by discs of millboard placed in the bottoms of the iron vessels. Before any regular or exact experiments were carried on with this battery, a few trials were made with it, to give an idea of its probable powers, some of which are the following:—

7. *Experiment* 1.—When six pairs were arranged in series, and charged with diluted sulphuric acid, the polar wires were properly connected with an electro-gasometer, whose terminal platinum plates are two-and-a-quarter inches high and one-and-a-quarter inch broad, consequently exposing a surface of upwards of eleven square inches to the acidulated water† in the instrument. The terminals gave off two cubic inches of the mixed gases per minute.

8. *Experiment* 2.—By adding two other pairs to the last series, and arranging the whole in a series of eight pairs, the terminals in the electro-gasometer liberated seven-and-a-half cubic inches of the mixed gases per minute. The above results were obtained several times over, and in some cases after the battery had been in action for more than three quarters of an hour.

9. *Experiment* 3.—The electro-gasometer was now laid aside, and the calorific effects of the eight pairs in series were as follow:—

* About a year previously, I formed a battery of twelve iron gas-tubes, each twelve inches high; and strips of amalgamated zinc, which performed well.

† The liquid in the electro-gasometer was six water and one sulphuric acid, by measure.

Charcoal gave out a small star of brilliant light.

One inch of copper wire, 1-25 of an inch diameter, was fused.

Four inches of ditto made white hot.

Eighteen inches of ditto made red hot in broad day-light.

Eight inches of watch main-spring was made red hot.

Two inches of ditto made white hot for several successive minutes.

10. *Experiment* 4.—The battery had now been in action more than an hour, and its decomposing powers were again ascertained to be equal to those exhibited at first, the terminal platinum plates still liberating the mixed gases at the rate of seven-and-a-half cubic inches per minute. The Voltaic series, on this occasion, was not extended beyond eight pairs, in consequence of the other two iron jars being leaky, and could not be used until the fissures were repaired.

11. *Experiment* 5.—As the Exhibition Gallery of this Institution was shortly to be opened to the public, I was requested by some of our Directors to try if this battery could be used to illustrate the explosions made by Colonel Pasley against the wreck of the *Royal George.* For this purpose, the series of eight pairs was furnished with two conducting wires, 200 feet in length each, making a circuit of 400 feet long. When the farthest extremities of these wires were joined by a thin platinum wire, the latter instantly became red hot, which left no doubt of the calorific powers of the battery being capable of exploding gunpowder at that distance; but, as no preparations had been made for trying its calorific effects below the surface of a body of water, nothing farther was done at that time.

12. *Experiment* 6.—On Saturday afternoon, the 30th of May, some of our Directors and a few other gentlemen met in the Gallery, and it was proposed to try the iron battery again; and, as the two leaky jars (10) were now repaired, the whole ten were arranged in one Voltaic series, and charged as before with diluted sulphuric acid. The electro-gasometer which had been used in the former experiments (7) having been broken by accident, another, of much larger dimensions, was now employed. Its terminal metals consist of two sheets of thin platinum, exposing about 144 square inches of surface to the acidulated water in the apparatus. When the ten pairs, in series, were properly connected with the terminals of this instrument, 15 cubic inches of the mixed gases were liberated per minute. In the course of about eight minutes' action, the rate of decomposition sank to about 13 cubic inches per minute; and, after a quarter of an hour's action, it became reduced to about 11 cubic inches per minute.

13. *Experiment* 7.—Preparations were now made for imitating the "blowing-up" of the *Royal George;* but as no water could be let into the basin of the canal in the Exhibition-room of the Institution, in consequence of the painters being at work in it, we had recourse to a very humble, and to some persons it will appear a most ridicu-

lous substitute, viz., a bucket of water. Our charge of gunpowder was the same as that used in the Polytechnic Institution, in London, being furnished with a stock of cartridges from Messrs. Watkins and Hill, Charing Cross, which had been made for similar illustrations in that Institution. The bucket of water being placed on the floor of the Lecture-room, and one of the extremities of each long conducting wire (11) being twisted to the wires of the cartridge, the other extremity of one of them was attached to one pole of the battery, situated in the passage outside of the room door. When the word "fire" was given, and the circuit completed by Mr. Brookhouse, who stood by the battery with the other connecting wire for that purpose, the most singular phenomenon occurred that was ever beheld by any of the party present, and certainly one which none of us had been led to expect. The explosion of the gunpowder was accompanied by a simultaneous perpendicular ascent of both bucket and water into the air, where they seemed to rest for a moment, at an altitude of about five-and-a-half feet above the floor, when they both fell, and the greater part of the water spilled on the floor. The singularity of this antic of the bucket produced an effect on the bystanders more easy to imagine than describe: every one involuntarily burst into an immoderate fit of laughter, which became more and more excited as each person described the ludicrousness of the event; and the consternation displayed by the two servants who were present in finding mops, basins, and other paraphernalia, with which they were not prepared, for taking up the water from the room floor, added no little to the burlesque character of the scene. However, the two men were very active, and in a short time most of the water was in the bucket again.

14. *Experiment* 8.—When the effect of the last "blow-up" had sufficiently abated, one of our Directors proposed that the experiment should be repeated, in order to ascertain how high the bucket and water could be raised by a second explosion. The necessary preparations being made, and chairs, forms, tables, &c. being removed from the vicinity of the bucket—the glass cupboard in which our splendid electrical machine is placed being guarded by chairs, forms, &c. against the effects of splinters, in case of the bucket giving way to the force of the powder, and the faces of glazed pictures being turned to the wall, &c.—the cartridge was sunk in the water, and on the word "fire" being given the explosion again took place. The bucket jumped up to the height of about four-and-a-half feet from the floor on to the lecture table, carrying with it only a small portion of water, the rest being scattered about in every direction. The servants, who were prepared on this occasion to take up the water from the floor, set to work with great alacrity in hopes to be enabled to replace the greater part of it in the bucket in a few minutes; but observing, after working a short time, that with all their efforts they were not lessening the water on the floor, one of them looked to see how much had been collected in the bucket, and immediately called out that "the

bottom was blown out!" Nothing better than this news could possibly have happened to give increased tension to the already excited risibility of the company.

15. The cause of the bucket and its water jumping up together by the first explosion may probably be traced to the sudden reaction of the floor against the bottom of the bucket, which rebounded with a force nearly equal to that with which the water was blown upwards, and, being in the same direction, they kept pace with one another.

16. *Experiment* 9. The battery had now been charged more than an hour, and its decomposing powers were again tried with the same electro-gasometer as last used. From a mean of several trials the liberated gases amounted to more than 10 cubic inches per minute.

17. Since the appearance of my pamphlet in 1830, experimenters have turned their attention to the improvement of Voltaic batteries, and several kinds have been invented, each of which has its peculiarities; and, for some processes, most of them have a great advantage over those previously in common use. It seems rather doubtful, however, from the facts hitherto in our possession, that we shall ever discover a form of battery capable of exhibiting every class of electric phenomena to the best advantage. It is true that with the command of an extensive series of moveable combinations or pairs, we can arrange them in groups or in series in a great variety of ways, and thus be enabled to modify their forces so as to become advantageously available for the display of the electro-magnetic, electro-chemical, and the electro-calorific classes of phenomena; but for the display of the purely electrical phenomena, such as the attractions and the repulsions, and the charging of coated glass, the original pile of Volta stands pre-eminent; and, amongst all the forms of *battery* which have hitherto made their appearance, that of Cruickshanks is the only one which can be advantageously employed for purposes of this kind, and for medical treatment it seems better adapted than any other.

18. The batteries severally invented by Grove and Smee are, unquestionably, about the most powerful now generally known for continued action in the electro-magnetic, electro-chemical, and electro-calorific departments; but their high price almost precludes their general employment amongst experimenters, excepting in such cases as where the funds of an institution are at command. Professor Daniell's battery is also so constructed as to retain its powers in action for a long time together; but, unless of large dimensions, its chemical, magnetic, and calorific powers are far below those of the two former batteries. Besides the first cost of Grove's and Daniell's batteries, there is a continual current expense attending their preparation and keeping in order for experiment, to which Smee's battery is not subject; for diluted sulphuric acid being the only liquid used, and having no diaphrams between the metals, the excitation is accomplished at a cheap rate, and is not complicated by appendages which are

expensive in every form they have hitherto assumed, not only in the first purchase of the battery, but by the frequent renewal of those which become destroyed, and the time necessarily required for their preparation.

19. Notwithstanding the advantages obtained by the great superiority in the action of the modern forms of battery over that exhibited by those invented respectively by Cruickshank and Wollaston, but very little seems to have been done towards ascertaining their real capabilities, as to the most advantageous display of the several classes of phenomena to which they are best adapted: hence it is that their full powers are but little, if at all known. It is thus that an important inquiry is still left untouched, which may probably reveal facts of the highest interest to this department of physical science. Moreover, as the employment of Voltaic batteries has now become very extensive, not only in investigations but in the daily illustrations at this and many other similar Institutions, and is likely to be still more extensively employed both in military and civil engineering, it is obvious that a cheap, efficient battery, with the mode of conducting it to the best advantage, are desiderata of great moment to the practical man who may have occasion to avail himself of the advantages which such an implement affords in the daily processes of his professional avocations. But an investigation, such as is best adapted to reveal these important facts, would require the command of every kind of battery that appears likely to be adapted for general purposes, to which such an implement is peculiarly applicable; and although not much skill in manipulation would be absolutely essential to such an undertaking, the requisite series of experiments would be somewhat expensive, and could not be conducted without a considerable occupation of time.

20. The batteries belonging to this Institution are the following, viz.:—Cruickshank's, two troughs of 50 pairs of 3 inch plates each; Wollaston's, two troughs of 10 pairs of 4 inch plates, with double coppers each; Daniell's, 20 copper cylindrical jars, 24 inches high and 4 inches diameter, with amalgamated strips of rolled zinc, in hempen bags or diaphrams; Grove's, 50 pairs of 4-inch platinum plates, with double amalgamated zinc in porous pots for diaphrams. Besides these, we have 30 of those cast iron jars, with their amalgamated zinc cylinders, already described (6), and 20 pairs of copper and amalgamated zinc cylinders in porcelain jars. I have availed myself of the use of these batteries, and also of one of Smee's construction of 12 pairs, which, by the kindness of Mr. Joseph Lockett, has been placed in my hands, for the purpose of comparing their powers in the display of electro-chemical, electro-magnetic, and electro-calorific classes of phenomena, and for ascertaining which kind of battery is most likely to become more generally useful, both as regards economy and facility of manipulation.

On the Chemical Powers of Voltaic Batteries.

21. The chemical powers of our modern batteries have hitherto been tested in no other way than by the decomposition of acidulated water. This circumstance may

probably be owing to the great facilities which are afforded by operating on this compound, and the supposed exactness of the results. In point of preparation and manipulation, there can be no doubt of the superior facilities for the decomposition of water over that of most other bodies; but, notwithstanding the facilities thus afforded to experimenters, the decomposition of water, as a test for the powers of Voltaic batteries, has led many to the most extravagant inaccuracies; and I am not aware that any experiments are on record that have been directed to an inquiry for ascertaining the best means of arriving at a maximum of decomposition by the employment of any one of the several batteries which have hitherto been constructed. Attempts have not been wanting, however, to introduce arbitrary standards of admeasurement of the relative powers of Voltaic batteries. Heating metallic wires, deflections of magnetic needles, and decomposition of certain liquid compounds, have, *unsuccessfully*, been resorted to for this purpose. Unfortunately, also, the errors of celebrious men, whatever be the nature of their pursuits, are almost sure to lead those astray who have no means or no opportunity of judging for themselves; and in this respect no contrivance appears more eminently calculated to mislead the unwary experimenter, than that called the *Voltameter.—Philosophical Transactions*, 1834.

22. If we wish to arrive at a knowledge of the powers of any Voltaic battery in the process of decomposing water, there are several particulars which are necessary to be attended to, some of which will vary with almost every form of battery, whilst others are common to all batteries whatever. The first essential point to be determined is, which is the most influential body in facilitating decomposition when dissolved in the water to be operated on? And as that solution which facilitates decomposition the most in one case will also facilitate it to the greatest extent in all, whatever may be the form of battery employed, the determination of this point becomes easily accomplished. A solution of sulphuric acid is now generally placed in connection with the platinum terminals in the decomposing apparatus, and answers very well for these inquiries, especially when the water is to the acid as about five to one. Whatever may be the real character of the action of bodies which facilitates the decomposition of water—whether it be a mere mechanical separation of its particles, which makes them more assailable to the electric forces—an improvement in its electro-conduction, and thus permits the introduction and consequent flow of a greater quantity of electric fluid; or whether it admits of an improved electro-polarization by an association with the particles of the dissolved body, remains a problem for which philosophers have not yet found a solution.

23. The second consideration is the *distance* between the platinum terminals in the decomposing apparatus, which can hardly be too small, provided they do not touch one another. This is a fact generally known, and like the former particular applies to all batteries whatever.

24. The third thing to be determined in the decomposition of water is the *size* of the terminal metals in the decomposing apparatus; for the extent of decomposition will vary very considerably with terminals of different extent of surfaces. With feeble batteries, it is necessary to cencentrate the electric force to a mere point before any decomposition of water can be accomplished; hence, in such cases, short, thin platinum wires are preferable to the terminals of larger dimensions. The decomposition of water, however, is not the best test for ascertaining this law with precision, when the intensity of the battery is very feeble. Perhaps the following experiment will answer as well as any.

25. *Experiment* 10.—Employ a battery of one pair only of small dimensions, and let the liquid operated on be a strong solution of sulphate of copper. Let the terminal metals be sheets of platinum foil of three or four square inches each, and immerse them both completely in the cuprous solution. No decomposition is perceptible, even though the connection be continued for more than an hour; but a Galvanometer placed in the circuit indicates the existence of a current. Let now the negative terminal be taken out and wiped dry, and then immerse only one of its corners: in a few minutes the immersed corner will be covered with precipitated copper, indicating decomposition by the force of the concentrated current at that point; but the Galvanometer needle indicates a much feebler general current than when the platinum plate was wholly immersed. By immersing the corner of the platinum terminal to different depths in the solution, the exact amount of metallic surface which just allows of decomposition may be discovered. And it will be found in all cases, that as the immersed surface increases, the magnetic deflections increase also. Hence it becomes obvious that the powers which such feeble currents exercise on a magnetic needle are no indications of the chemical powers of the battery; unless, indeed, we look for the one as the reverse of the other. There are several interesting facts on this nice subject; but as the principal object of this Memoir is to investigate the powers of the most formidable batteries known, I shall not dwell upon them until a future opportunity presents itself.

26. The fourth point to be determined to effect the maximum of decomposition of water, by Voltaic Electricity, is *the proper extent of the Voltaic series*, or of the proper unit of *intensity* of the battery; and as the intensities of different batteries, with the same extent of series, differ very much from each other, the determination of this point must be of great interest to experimenters generally.

27. Having now pointed out four grand particulars to be attended to for obtaining a maximum decomposition of water by Voltaic Electricity, I will next proceed to describe the results of a few series of experiments made with the various kinds of batteries already noticed.

Table of Experiments on the Decomposition of Water, with various Series of Professor Daniell's Voltaic Battery, with the two Electro-Gasometers described in paragraphs 7 *and* 12 *:*

No. of Pairs in Series.	Quantity of Gas obtained per Minute.		No. of Pairs in Series.	Quantity of Gas obtained per Minute.	
	From the Large Terminals.	From the Small Terminals.		From the Large Terminals.	From the Small Terminals.
10	$9\frac{1}{2}$	9	5	$5\frac{1}{4}$	5
9	9+	$8\frac{1}{4}$	4	$3\frac{1}{2}$	$3\frac{1}{4}$
8	$7\frac{1}{2}$	$7\frac{1}{4}$	3	2	$1\frac{1}{4}$
7	7−	$6\frac{1}{2}$	2	Scarcely any from either.	
6	$5\frac{3}{4}$	$5\frac{1}{4}$			

Each of the above tabulated results is the mean of several trials; they furnish us with a knowledge of the *unit* of *intensity* of this kind of battery, which is obviously that given by a series of five pairs. And although the decomposition by an extensive battery would not suffer much loss by employing a series of either six or seven pairs, yet any series above seven, or below five, would be attended with a great loss in the *quantity* of decomposition in a given time. Another essential feature in these results is in the quantities of gas liberated by the different sized terminals—the larger ones invariably producing the greater quantity.

In another series of experiments with Mr. Daniell's battery, and the electro-gasometer with the larger plates (12), I obtained $10\frac{1}{2}$ cubic inches of the mixed gases per minute, with a series of ten pairs; and with lower series, the rate of decomposition was nearly proportional to that in the above table; thus indicating by both sets of experiments, that the proper unit of intensity is a series of five pairs, for by employing the ten pairs, in two series of five pairs each, I obtained above 12 cubic inches of the gases per minute.

29. *Table of Experiments on the Decomposition of Water, by various Series of Voltaic Pairs of Cast Iron and Amalgamated Zinc, as described in paragraph* 6 *:*

No. of Iron Jars in series.	Cubic Inches obtained per Minute.		No. of Iron Jars in series.	Cubic Inches obtained per Minute.	
	Large Terminals.	Small Terminals.		Large Terminals.	Small Terminals.
10	14	10	6	4	$2\frac{3}{4}$
9	11	$8\frac{3}{4}$	5	$2\frac{1}{2}$	2
8	10	7	4	1	$\frac{1}{4}$
7	$7\frac{1}{4}$	5	3	Scarcely any.	

30. The first thing to be observed in this table, is the superiority of action by the large terminals, over that by the smaller ones; and in a much greater degree than by Daniell's form of battery. The next thing to be observed, is the rapid increase of decomposition, by an incrase of the Voltaic series, even up to ten pairs, by which we understand that the whole in one series is much more powerful than in any other way we could combine them; and it is probable that, by extending the series, we should discover that the proper unit of intensity is considerably greater than that given by ten pairs.

31. The above results were by the employment of the first ten pairs of this kind that were constructed; but, since the time the above experiments were made, I have obtained 22 cubic inches of the mixed gases per minute with the ten pairs in series; I have also had 20 new iron jars cast, with ten pairs of which I have obtained 99 cubic inches of the gases in four minutes action, and I am in hopes of arriving at a still greater rate of decomposition. In all cases, with the iron batteries, the decomposition has increased rapidly up to ten pairs, in series, indicating that a still higher intensity is required for the most advantageous *unit of intensity.*

Table of Experiments on the Decomposition of Water, by various series of Voltaic Pairs, on the principle of Mr. Smee's Battery. The Electro-Gasometer, with large terminals (12) *was the only one employed in this series of experiments.*

No. of Pairs in series.	Cubic Inches of Gases liberated in One Minute with large Terminals.	No. of Pairs in series.	Cubic Inches of Gases liberated in One Minute with large Terminals.
2	Scarcely perceptible.	7	8
3	Ditto.	8	11
4	$\frac{1}{3}$	9	13
5	$1\frac{1}{2}$	10	15
6	3		

32. If we look to the rapid increase of decomposition, from a series of six pairs to the series of ten pairs, we are soon convinced that a series of ten is more advantgeous than any series below that number; and it is very probable that the proper *unit* of intensity with this battery, as with the cast iron one, is considerably above that given by a series of ten pairs. This point, however, must be determined by future experiments, as I have not, at present, more than ten pairs at command. But the experiments detailed in the above table will be a sufficient guide for the present, for any person employing no more than ten pairs at once, because it is obvious that the decomposition of water will be accomplished to the greatest extent by employing them in one series; which also appears to be the case with the cast iron battery.

33. *Experiments on the Decomposition of Water, by various series of Voltaic Pairs, upon the principle of Mr. Grove's Battery. The decomposing apparatus with the larger terminals* (12) *was used.*

No. of Pairs in series.	Cubic Inches of Gas per Minute.	No. of Pairs in series.	Cubic Inches of Gas per Minute.
2	Scarcely any	7	16
3	6	8	18
4	9	9	21
5	11	10	24
6	14		

34. From the results of this series of experiments, it is obvious that the 10 pairs in series produce more decomposition than by any other combination of them; and it is probable that a still more extensive series would be the proper *unit* of *intensity* for accomplishing the maximum of decomposition by this kind of battery. Mr. Grove has, I believe, constantly employed his battery in series of five pairs only, which series is obviously too small, and occasions a considerable loss of decomposing power.

35. Suppose, for instance, that a battery of 30 pairs were to be used in six series of five pairs each, then as five pairs give 11 cubic inches of gas, $5 \times 6 = 30$ pairs would give $6 \times 11 = 66$ cubic inches. But 30 pairs in three series of 10 pairs each would give $3 \times 24 = 72$ cubic inches of gas, which is six cubic inches more than by Mr. Grove's mode of combination.

36. In order to compare the decomposing powers of these batteries, it will be necessary to ascertain their *relative* metallic surfaces exposed to the exciting media. They stand as below for each pair:—

Daniell's . . .	= 360	square inches	of metallic surface.
Smee's . . .	= 192	ditto	ditto
Sturgeon's . .	= 162	ditto	ditto
Grove's . . .	= 104	ditto	ditto

37. Thus, by assuming Mr. Grove's battery as the unit of surface, and also the standard of decomposing power, we shall have:—

Metal.		Gas.		Metal.		Gas.	
104	:	—	: :	—	:	24	Grove's.
162	:	25	: :	104	:	14·8	Sturgeon's.
192	:	15	: :	104	:	8·1	Smee's
360	:	12	: :	104	:	3·5	Daniell's.

38. Hence it appears, that if the whole of the batteries exposed precisely the same extent of metallic surface to the existing liquid, that invented by Mr. Grove would have a decided preference, and Professor Daniell's battery would hold but a very low rank in point of decomposing power. But if we view them individually according to their respective sizes in which they have been employed in these experiments, then their maximum powers that I have obtained, will stand thus:—

Sturgeon's	25 Cubic inches of gas	per minute.
Grove's	24 ditto	ditto
Smee's	15 ditto	ditto
Daniell's	12 ditto	ditto

39. The next consideration is the cost of these batteries, both as relating to the first purchase and the current expense of keeping them in action. The price given for 12 pairs of Smee's construction, Mr. Lockett informs me, was £32; hence the price of 10 pairs would be £26 13s.* The price of 10 pairs of each of the other kind of batteries is, Grove's £7; Daniell's £6; Sturgeon's £3 10s.

40. The excitation is carried on by about the same quantity of sulphuric acid in each battery; and in Smee's, and the iron batteries, no other expense is required. But in Grove's battery 1½ lbs. of the best nitric acid for 10 pairs is used in addition; and in Daniell's, about 5 lbs. of sulphate of copper, in addition to the sulphuric acid, is used for 10 pairs. In both these latter batteries, there are also diaphrams which are continually falling into decay, which is another current expense attending these batteries. The mercury employed in the amalgamation of the zinc would be nearly the same in all the forms of battery hitherto described, but the time occupied in fitting up is very different indeed—the iron battery requiring much less time than any of the other forms. Hence, as far as the decomposition of water is concerned, the iron battery has a decided advantage, both in point of power and economy, and is so simple that it is manageable by any person: and what is another point in its favour, it works best when quite rusty, and retains its power a long time. The hydrogen is certainly an annoyance, but I have hit upon a contrivance to remove it, which will be described in the sequel.

41. It has already been stated (31) that 20 additional iron jars were procured to the former 10, and having also at command 50 pairs of Grove's battery, I proceeded to increase the series of each of these two kinds of battery, in order, if possible, to trace the decomposing action to the maximum series or *unit of intensity* of each kind, by which they may be employed to the greatest advantage in extensive arrangements, or when a great number of pairs are about to be used. In demonstrations before classes, and

* There can be no question of this being a very extravagant price, as I am confident that it can be had for less than half that sum.

in lectures where the fact requires no further experimental illustration than by the usual way of exhibition, the decomposition of water by ten pairs will be found amply sufficient, even in the most spacious lecture-room; for when we can command 25 cubic inches of the mixed gases per minute a tolerable large receiver may be completely filled in a very short time; which forms a most surprising contrast to the puny results obtained by means of our earliest forms of battery. But, as on some occasions, we are desirous of showing the electro-decomposition of water to the greatest extent which our batteries are capable of performing, it will be interesting to know in what way to arrange them to the best advantage in the display of this phenomenon.

42. The two following tables will show the results with various series from 10 to 20 pairs of each kind of battery:—

Table of Experiments on the Decomposition of Water, by various series of Voltaic Pairs, on the principle of Mr. Grove's Battery (20). *The Electro-gasometer, with large terminals* (12) *was the only one used in these experiments.*

No. of Pairs in series.	Cubic Inches of Gas per minute.	No. of Pairs in series.	Cubic Inches of Gas per minute.
10	14	16	10
11	13	17	10
12	12	18	10
13	12	19	10
14	11	20	10
15	11		

43. The results in the above table show that a series of about 10 pairs is the proper unit for obtaining a maximum of decomposition of acidulated water; and that to employ either more or less would be attended with a considerable loss of action. The action generally is much less in this series of experiments than in those described in (33;) but that circumstance could have but little effect on the result of the present inquiry.

44. *Table of Experiments on the decomposition of Water, by various series of Voltaic Pairs, with the Cast-iron Battery* (31). *The large Electro-gasometer was used in these Experiments.*

No. of Pairs in Series.	Cubit inches of Gas per minute.	No. of Pairs in Series.	Cubit inches of Gas per minute.
10	24	16	22
11	27	17	18
12	31	18	17.5
13	28	19	17
14	23	20	16
15	22		

45. The results of these experiments are highly valuable, both in a theoretical and a practical point of view, indicating, as they do, that a series of 12 pairs is not only the proper unit for obtaining a maximum of decomposition, but that a more extensive series is absolutely detrimental, and will not give so much gas as by the employment of 12 pairs only.

Indeed, it is a most curious fact that 20 pairs in series will only produce two-thirds of the decomposition that 10 pairs in series will do. From this fact it would appear probable that we might increase the series of pairs to such an extent that they would so far neutralize the chemical action of the battery, as to render it incapable of decomposing acidulated water.

46. By combining 24 pairs of the iron battery into two series of 12 pairs each, in such a manner that both series should operate in concert on the acidulated water in the large decomposing apparatus, I was enabled to obtain an average of 60 cubic inches of the mixed gases per minute. The action of this battery, like that of all others, varies according to the condition of the zinc surfaces, being always the most powerful when those surfaces are smooth and well amalgamated. With new zincs, well amalgamated, I have obtained 64 cubic inches of the mixed gases per minute, with 24 pairs in two series of 12 pairs each, operating in concert.

47. There is another circumstance yet to be noticed in the decomposition of acidulated water; and it is probable, I think, that the same circumstance may have an influence on other compounds. The large decomposing apparatus which I employed in these experiments has one of its platinum terminals about twice the size of the other; and when the larger one is connected with the positive pole of the battery, the decomposition of the water is carried on to a greater extent than when the connections are made in the opposite way. In some cases, a difference of two cubic inches of gas per minute is obtained by the action of a battery of 10 pairs. Hence it will be necessary to mention, that in all the preceding experiments the positive pole of each battery was connected with the larger platinum terminal, and consequently the negative pole with the smaller terminal. I am very far from supposing that even these large terminals are sufficiently extensive for showing the maximum of decomposing power of any of the several batteries which I have employed. I think it probable that, by employing terminal surfaces at least twice as extensive as those in the larger decomposing apparatus (12), a considerable increase of decomposition would be obtained—although, for those batteries which do not afford much power, these terminals would be much too large. Hence it would be in vain to look for any one *pair* of terminal metals that would afford a maximum of decomposition for every kind of battery; and the fallacy of any indications of the powers of large batteries, which can be obtained by the decomposition of acidulated water from *small terminal wires*, is now too obvious to require any farther investigations respecting it.

Moreover, since the decomposition of water is a phenomenon of one particular class only, and that other classes of phenomena, quite as important as the electro-chemical, are displayed to the greatest advantage by very different arrangements of Voltaic batteries to that which gives a maximum of chemical action, it would be absurd in the extreme to continue the term " *Voltameter*" to any piece of apparatus which does not indicate the powers of Voltaic batteries, in the production of even *one class* of phenomena; and which gives no idea whatever respecting the powers of batteries in the production of other classes. It is well known to the scientific world that I have given to the water-decomposing apparatus the name of " *Electro-Gasometer*," a term which cannot possibly lead any one into error, because it shows the absolute quantity of gas liberated by each battery employed, and presumes nothing more; and as I have shown that the quantity of gas liberated by any *individual* battery will depend on the extent of surface which the platinum terminals exposes to the water in the instrument, it is obvious that no individual electro-gasometer can indicate the maxima of decompositions which different kinds of Voltaic batteries are capable of displaying. The instrument which we continue to call a " *Galvanometer*" is in precisely the same predicament as the Voltameter, because it indicates nothing more than electro-magnetic deflections. The proper name for this instrument would be " *The Electro-Magnetometer;*" but on this point I shall not dwell at present, as I shall shortly have an opportunity of interweaving its investigation into others which will form the subject of another Memoir.

The Calorific Effects.

48. With respect to the calorific class of phenomena, I have not been able to prosecute my inquiries to a sufficient extent to ascertain the relative powers of the batteries already described. The experiments which I have hitherto made for this purpose have been with comparatively short conductors, such as are usually employed at the lecture table, or for illustrations in our large Exhibition-room. Through a circuit of 200 feet of copper wire, of 1-12th of an inch diameter, a series of ten pairs of the iron battery ignites platinum for the explosions of gunpowder as decidedly as by the employment of any other battery whatever; but I am not aware of the maximum distance from the battery at which gunpowder could be exploded by the calorific powers of ten pairs, but there can be no doubt of a series of ten pairs accomplishing the ignition of gunpowder at a much greater distance than 200 feet, if the copper conductors were sufficiently thick. I believe that Colonel Pasley employs copper conductors of half-an-inch diameter, which, perhaps, is not too much for facilitating the propagation of the electro-calorific powers through long conductors. By the employment of such capacious conductors, the iron battery will be found to answer

all the purposes of springing mines, blasting rocks, &c., or in any other capacity in which gunpowder is to be exploded by Voltaic Electricity. The *quantity* of Electricity excited by this battery is very great, and its propelling powers may be increased by the addition of a few pairs to the series.

49. In the Exhibition Gallery of this Institution we proceed through our daily illustrations of the decomposition of water, the ignition of metals, and Colonel Pasley's operations against the wreck of the Royal George, with an iron Battery of eight pairs only. For the ignition of short, thick metallic wires, the iron battery is far superior to any of the others: when operating on long, thin wires of platinum it is not so good as Grove's, though for the ignition of copper wires, which are better conductors, it is still superior. Hence there is an immence *quantity* of Electricity yielded by the iron battery, but it is not of high intensity.

Description of a Series of the Iron Battery.

50. When an extensive series of the iron battery is employed, there is a considerable quantity of hydrogen gas liberated at the surface of the iron, which is an annoyance from which Daniell's and Smee's are free; but by arranging them under a rectangular cover or hood of japanned tin or zinc (as in Fig. 2, Plate VII.) the hydrogen is prevented from making its escape into the room, and the operator experiences no inconvenience whatever. The hood rests on a stout board, round the upper side of which a deep groove is made for the reception of the lower edge. The hydrogen may be disposed of as fast as it rises from the battery, by connecting one end of a flexible tube to the top of the hood, as in Fig. 2, and placing the other end under the chimney flue. Fig. 1, shows one of the iron jars with its zinc cylinder attached. Fig. 2, represents a series of eight pairs, covered with the hood, and connected with the electro-gasometer, by means of two stout copper wires, which pass through the board, and rise sufficiently high above the surface to be united (within the hood) with the poles of the battery. The platinum terminals are in the glass vessel nearest to the battery, which has a bent tube attached to its neck, for the purpose of conveying the gases to the glass reservoir, *r*, which stands inverted in a trough of water.

Electro-Magnetic Phenomena.

51. The relative electro-magnetic powers of any two batteries of large dimensions, and in vigorous action, are not easily ascertained on small masses of iron, because even the feebler battery may be of sufficient power to magnetize the iron to its maximum point. But by operating on large pieces of iron, or on moderate sized ones,

with a single pair of Voltaic metals, of small demensions, we have a pretty fair opportunity of arriving at decisive results.

The electro-magnet, belonging to this Institution, is made of a cylindrical bar of soft iron, bent into the form of a horse-shoe magnet, having the two branches parallel to each other, and at the distance of 4·5 inches. The diameter of the iron is 2·75 inches; it is 18 inches long when bent. It is surrounded by fourteen coils of copper wire, seven on each branch. The wire, which constitutes the coils, is 1-12th of an inch diameter, and in each coil there are about seventy feet of wire. They are united in the usual way, with branch wires, for the purpose of conducting the currents from the battery. The magnet was made by Mr. Nesbit. With this magnet, and a battery of Professor Daniell's construction, we have obtained the following results:—

Experiment.	Number of cells.	Weight sustained.	Weight which broke off keeper.
1	19 in series	10 cwt.	$10\frac{1}{2}$ cwt.
2	19 do.	$11\frac{1}{2}$	12
3	1 do.	$8\frac{1}{2}$	9
4	10 do.	10	$10\frac{1}{2}$
5	10 do.	$11\frac{1}{2}$	12
6	16 in series of 4 each.	12	$12\frac{1}{2}$
7	16 do.	$12\frac{3}{4}$	13

52. The greatest weight sustained by the magnet, in these experiments, is $12\frac{3}{4}$ cwt. or 1386 lbs., which was accomplished by sixteen pairs of plates, in four groups, of 4 pairs in series each. The lifting power by 19 pairs in series was considerably less than by 10 pairs in series; and but very little greater than that given by one cell or one pair only. This is somewhat remarkable, and shows how easily we may be led to waste the magnetic powers of batteries by an injudicious arrangement of its elements.

53. Two experiments were then made with the electro-magnet (51), and a single pair of metals, excited by a strong solution of nitric acid. The metals were copper and rolled zinc, each formed into a cylindrical scroll and placed in a porcelain jar, eight inches high and five inches diameter. The first experiment was made when the acid solution covered only one inch high of the lower edges of the metals; and the second experiment when the pot was filled to the brim.

	Weight sustained.	Weight which broke off keeper.
First experiment	$8\frac{1}{2}$ cwt.	9 cwt.
Second ditto.	10 „	$10\frac{1}{2}$ „

In the first of these experiments, the Voltaic metals in action exposed a surface of about 40 square inches to the acid solution, and the weight sustained was precisely the same as that sustained by using one pair of Daniell's form, which exposes a

metallic surface of about 360 square inches. Indeed, I have always found that the most vigorous magnetic action of any battery is excited by acid solutions; and if a pretty good share of sulphuric acid be not used in Daniell's battery, its action in the display of both chemical, magnetic, and calorific phenomena is exceedingly low.

54. *Experiments made with the Electro-Magnet* (51) *and the Cast Iron Battery* (50).

No. of pairs in series.	Weight sustained.	Weight which broke off the keeper.
8	$12\frac{1}{2}$ cwt.	13 cwt.
4	13 ,,	$13\frac{1}{2}$,,
Single pair	$10\frac{1}{2}$,,	11 ,,

By comparing the two sets of experiments, it will be found that the iron battery has a considerably greater magnetizing power than Professor Daniell's form, both when in series and in single pairs.

55. By employing iron for one of the Voltaic metals, we have an opportunity of making a battery and an electro-magnet of the self-same materials; or, if you please, of converting the battery into a magnet. A battery-magnet of this kind, which I made some time ago, is represented in Fig. 3, Plate VII. It consists of two short pieces of musket barrel, the lower end of each of which is plugged up by a solid piece of iron, and thus welded together to make the plug and barrel one piece of iron. The upper ends of the tubes are joined together by a cross-piece of flat iron, through which they pass, as seen in the figure; and the lower ends are filed flat and smooth, for close adaptation to the keeper. Close to the lower end of each branch is soldered one end of a covered copper wire, about 1-10th of an inch in diameter, which is afterwards coiled round the barrel to the top, where it terminates in a cup for holding mercury. A narrow slip of amalgamated rolled zinc, with its connecting wire, is then placed in each barrel, and the connections made as seen in the figure. When charged with diluted sulphuric acid, this battery-magnet will carry about sixty pounds.

Electro-Magnetic Telegraphs.

56. In consequence of the various practical applications of the Voltaic battery, it has become a valuable implement to society, and every attention of the experimental philosopher is required to render it effective for the various purposes to which it is applied. The miner, the chemist, the statuary, the artist, and the engineer have availed themselves of the powers of the Voltaic battery; and the telegraphs of the present day, in all their various forms, have their main-springs in the Voltaic battery.

57. The soft iron electro-magnet also, the first forms of which are represented by Figs. 13 and 14 of Plate IV. and Fig. 1, Plate V. derives its powers through the

instrumentality of the Voltaic battery; and has become, as decidedly as the battery itself, an indispensible apparatus in the structure of modern telegraphs—in some cases merely to awaken the attention at a distant station, whilst in others the electro-magnet performs the whole of the telegraphic movements, whether these movements themselves, and alone, be the concerted emblems of words, or that the transmitted intelligence be imprinted or written in permanent characters.

58. Notwithstanding the several ingenious forms of telegraph that have claimed public attention, there are yet many ways of applying electro-magnetic forces which appear equally efficient, for telegraphic purposes, as any that have hitherto appeared. During the last and present years I made several forms of telegraph perfectly distinct from any of those previously known. Some of them are described in the fifth volume of the *Annals of Electricity, &c.;* and in all of them the electro-magnet forms an essential part in their structure and operations.

59. The operative part of one of these telegraphs is represented by Figs. 4 and 5, Plate VII. In Fig. 5, N M S represents an electro-magnet of the horse-shoe form; and the part of Fig. 4, marked M, is an edge view of it. A piece of iron, C C, attached to the short arm, O F, of a lever, O L, Fig 4, is ready to be attracted and drawn down to the magnet as soon as its powers are developed by the action of the battery, and as speedily released from it when the battery action is cut off. The longer arm of the lever carries a light disc, on which is painted either a letter or figure for a signal; which, being situated behind a dial-plate, cannot be seen only when opposite to a hole in the plate, at which it presents itself when the iron, C C, is drawn down to the magnet. The lever moves on a fine steel axle at F. Either one or more of these magnets might be employed, according to the requirements of the telegraph; but, as the action of the magnet can move the lever in one direction only, those forms of telegraph next to be described, each having two motions of the same index, will necessarily be more efficient, and consequently the more likely to become of public utility.

60. Fig. 6, Plate VII. is a perspective representation of a telegraph, consisting of two horse-shoe electro-magnets, and a permanent steel magnetic bar placed between them. The electro-magnets are fixed horizontally to a base-board, and the steel bar is attached to the lower extremity of a vertical lever, O L, which moves on an axle at A. The same conducting wire forms the spirals to both electro-magnets, and in such a manner that those ends of the latter which are opposite to each other shall display poles of opposite characters whenever the battery connections are completed. Hence it is obvious, by mere inspection of the figure, that, by one connection with the battery, the magnetic steel bar will be drawn towards the left hand electro-magnet, N U S, and at the same time it will be repelled from the right hand magnet, N M S; but when the battery connections are reversed, and consequently the magnetic poles reversed also, the suspended steel bar will be drawn towards the right hand magnet

and repelled by the other. Thus the motions of the short arm of the lever, to which the bar-magnet is attached, will be urged, by four magnetic forces, either to the right or to the left, whenever the battery is brought into play on the soft iron horse-shoes. The whole of these movements are situated behind the dial-plate; and the longer arm, *a l*, of the lever, *o a l*, being fixed on that end of the axle, *a*, which passes through the dial-plate, becomes the index, and may be made of any required length.

61. A very simple arrangement for telegraph movements is represented by Fig. 7, Plate VII. N M S is a soft iron electro-magnet, and N S a light permanent steel magnet or needle, moveable on a horizontal axis which passes through its centre, *c*. The same axis carries an index in front of the dial-plate. Now it is obvious that the motions of the needle to the right or the left will depend on the direction of the electric current through the spiral conductor which encloses the soft iron horse-shoe, because the character and situation of the magnetic poles of the latter will be determined accordingly. By placing either the north or the south pole of the needle directly between the poles of the electro-magnet, it is subject to a much stronger force from the latter than if placed in any other situation; and its motions are quick and prompt when even a very small battery is employed. When two or three electro-magnets are placed behind one another, so as to operate on as many needles on one axle, a great increase of power is obtained without any additional battery action. The same conducting wire applies to all the iron horse-shoe electro-magnets.

62. Fig. 8, Plate VII. represents the application of two electro-magnets to one needle. By this arrangement it will be seen that the electro-magnets are so contrived (by the spiral conductors) that the poles of the two electro-magnets, which are on the *same side* of the needle, are both of the same character, there being two north poles on one side and two south poles on the other. By these means both poles of the needle are acted on at the same time, one by each magnet, so that none of the forces are lost. In this form of telegraph, as in that last described, several needles on the same axle, with their corresponding pairs of magnets, would increase the efficience of the apparatus, and enhance the activity of the index in front of the dial-plate.

63. There is yet another form of telegraph which I have found to act very well, but it has not that simplicity of structure as those already described. It is represented by Fig. 9, Plate VII. The two electro-magnets, S P N and S M N, are fixed in the same vertical plane with the needle, but their poles are differently arranged to those in Fig. 8, having poles of different kinds presented to each other when the current passes through the conductor. On the axle which carries the index on the dial, there are fixed two small bar magnets, N S, S N, with their north poles outwards, and reaching to between the poles of the electro-magnets. These magnets are fixed at right angles to the index, the latter being vertical. Now, according to the arrangement shown in the figure, it will readily appear that the movements of the permanent

magnets and index will depend upon the direction of the electric current through the spiral conductors, so that a change in the latter would cause a corresponding change in the former.

Remarks.—In any of the above forms of telegraphs the electro-magnets might be replaced by permanent steel magnets, provided the moveable needles were of the electro-magnetic character; and in some of the forms, if not in all of them, this would be the better plan, because the permanent steel magnets might be employed to any required extent of power. The electro-magnetic needles act with great promptitude and activity when placed between the poles of a powerful horse-shoe magnet; and, if they be made of bundles of thin iron wire, they lose their polarity the moment the electric current is discontinued. By employing several of these needles on one axle, with a corresponding number of permanent steel magnets, similarly arranged to the electro-magnets in Figs. 7 or 8, we obtain a telegraph that will operate to greater distances, between station and station, than any now employed with any one battery whatever.

64. *The Alarm Bell.*—An improved method of calling attention to the telegraph at a distant station is much wanted. The clock-work of the present *alarm* has to be wound up at certain times, otherwise its bell remains silent; and it is well known that much time is often lost before the signal returns that "all's ready." The first expense of this piece of apparatus, as well as the occasional expense for repairs, might be entirely done away with, and the attention called at distant stations without delay, by the *direct* action of an electro-magnet.

65. Let N S, Fig. 10, represent an electro-magnet, and *i i* a piece of soft iron, in the capacity of *keeper* or cross-piece, which is supported on a steel spring, *i o*, fixed at its lower end. To the middle of the cross-piece is attached one end of a wire, which unites it with the spring shaft of a small hammer, *t h*; the head, *h*, of the hammer resting at a short distance from the fixed bell, B. Now when the battery connections are completed, the magnet, *n s*, draws the piece, *i i*, suddenly to its poles, and thus rings the bell: and when the electric current is cut off, the spring, *i o*, brings the piece, *i i*, to its original position. Now since the magnet can be made and unmade as fast as we please, the bell may be kept ringing for any length of time.

It must be observed, however, that the piece of iron, *i i*, must not be allowed to come into close contact with the magnetic poles, otherwise it might be retained after the electric current had left the spiral, for reasons shown in the fourth Memoir. A piece of card or thin leather, to prevent contact, will insure a prompt action of the apparatus. A ratchet wheel and spring, to *break* and *make* battery contact, is exceedingly convenient for this bell apparatus.

W. S.

Royal Victoria Gallery of Practical Science,
Manchester, 1840.

ON THE THEORY OF VOLTAIC ELECTRICITY.

EIGHTH MEMOIR.

1. Perhaps there is no individual branch of physical science that has exercised the ingenuity of philosophers to a greater extent, nor any one been more productive of diversity of opinion, as a theoretical topic, than Voltaic Electricity; and what may, perhaps, be considered still more remarkable, no theoretical discussion has been less successful in uniting the opinions of scientific men, so as to give a general sanction to any theoretical views that have hitherto been taken, from whatever quarter they may have emanated, than that which has proceeded from this subject; for, although it has now been continued through a long series of years, it still remains as inconclusive as at first.

2. Under these circumstances, therefore, any attempt that can be made in this place to elucidate the principles of a science in which the efforts of the highest minds have failed to produce a satisfactory explanation will necessarily appear under great disadvantages, with every probability of suffering the common fate of its predecessors; but as there are *some* points on which philosophers entertain the same opinion, there is still a possibility at least—though the attempt may be viewed in the character of a *forlorn hope*—of simplifying some others, so as to be sufficiently comprehended to gain general assent, and thus advance us one step farther into the apparent intricacies of the theory of Voltaic Electricity.

3. I believe that there is no difference of opinion, at the present day, respecting the identity of the agency in common Electricity and that which is productive of the phenomena in that branch of physics now under contemplation: hence it is that we are not misunderstood by the term "Voltaic Electricity," when represented as a peculiar branch of electrics. The generality of writers, however, on these matters make no distinction whatever between *Voltaic Electricity* and *Galvanic Electricity;* or, in other words, between *Voltaism* and *Galvanism*, although the sources from which the phenomena emanate are as distinct from each other as the ordinary electric machine is from either of them. Why this practice is not yet abandoned would not, perhaps, be easily determined; but it must proceed either from an ignorance of their distinction—from an unwarrantable proneness to confound the one with the other—or from a fear of stepping counter to custom, and thus avoiding the imputation of pedantry.

4. I am of opinion, however, that the omission of even a *pointed* distinction in "Voltaic Electricity" and "Galvanic Electricity," by writers of the present day, is not only a dereliction of duty towards their readers, who purchase and read their works under the impression of finding in them expressions of the clearest ideas that the science is capable of admitting, but the practice is fraught with the seeds of misconception and error. Therefore, whatever risk may be run on the score of pedantry, it is quite time that the practice of confounding the two sources of electric action with one another were laid aside, and that distinction resorted to which alone can lead to clear and unequivocal views, and facilitate the association of ideas with the true character of the source of action.

5. In the preface to Aldini's "*Account of the late Improvements in Galvanism*," published in the year 1803, the distinction between "Voltaism" and "Galvanism" would appear to be very clearly pointed out:—"A just tribute of applause has been bestowed on the celebrated Professor Volta for his late discovery, and I have no desire to deprive him of any part of that honour to which he is so justly entitled; but I am far from entertaining an idea that we ought on this account to neglect the first labours of Galvani. Though these two philosophers pursued different routes, they concurred to throw considerable light on the same points of science; and the question now is, to determine which of them deduced the most just consequences from the facts they observed, and then to ascertain whether the facts established by Galvani led to the theory of Volta, or whether those discovered by Volta are connected with the theory of Galvani. For my part, I am of opinion that these *two* theories may serve in an eminent degree to illustrate each other. Last year Professor Volta announced to the public the action of the *metallic* pile. I here propose to exhibit, according to the principles advanced by Professor Galvani, the action of the *animal* pile." And again, "In the first part of this work, I shall exhibit the action of Galvanism *independently of metals*, and explain some of its general properties."

6. It is singular enough, however, that Aldini, who was the nephew of Galvani, and who laboured hard to show the distinction of the sources of Galvanic and Voltaic electric action, should eventually merge the one in the other, and call a series of "eighty plates of silver and zinc" a "*Galvanic pile*."*

7. The distinction between the sources of Galvanism and Voltaism is very marked, and susceptible of clear and unequivocal definition—the former being either a natural or an artificial association of *animal* matter, whether alive or dead, whilst the phenomena of Voltaism emanate from associations of metals and other inorganic bodies. Galvanism, therefore, comprehends all those phenomena developed by *animal* Electricity, and Voltaism comprehends those developed by the simple contact of inorganic bodies, whether solid or fluid.

* See Aldini's work, page, 218.

8. The electrical organs of the *Torpedo Gymnotus Electricus*, and the muscular and nervous systems in all animals, are natural associations of animal matter, constituting sources of *Galvanic* electrical action; and are as decidedly Galvanic arrangements as are the artificial piles of muscle and brain first arranged by M. La Grave, and shown to the Galvanic Society of Paris, and afterwards operated with by Aldini and various other philosophers.

9. In most of those electrical arrangements of inorganic matter hitherto formed, one at least of the bodies employed has been a metal, and an association of *two* metals is invariably employed in every arrangement from which much action is derived. But it must not be considered, from the hitherto invariable practice of employing metals in Voltaic electrical arrangements, that electric action is not derivable from associations of non-metallic bodies; for it is well known that charcoal will supply the place of a metal, and it may easily be shown that the contact of liquids alone will produce electric action. These and all other associations of inorganic matter are sources of *Voltaic Electricity.*

10. With respect to the character of the action in Voltaic associations, and the *modus operandi* by which they propagate electric currents, I consider that the original exciting force or agency is purely the electric, to which cause alone I hope to be enabled to trace the mode of excitement as well in the *wet* as in the *dry* pile.

11. The theoretical views of Electricity which I have taken are clearly described in the twenty-second Memoir,* and are those alone which it will be necessary to apply on the present occasion, in forming the basis of the theory of Voltaic Electricity, an explanation of the principles of which will be much facilitated by commencing with the simplest case, and referring the reader to all that is said in that Memoir respecting the unequable distribution of the electric fluid amongst the bodies composing the surface of the earth, and as far within its body as has hitherto been explored.

12. Since then, equal volumes of all the different kinds of matter, whilst surrounded by an equable electric pressure, are charged with different quantities of the electric matter, proportional to their susceptibilities of receiving it, it is obvious that those quantities would vary with every variation of the electric pressure, whether that variation of pressure were general on every side or partial only, by its limitation to one particular portion of the surface. Let us, for instance, apply this reasoning to the well-known experiment with Volta's plates of copper and zinc, than which I know of no simpler case. (See Fig. 1, Plate VIII.)

13. Prior to bringing the plates into contact with each other, each plate is surrounded by an equable electric pressure, and consequently each has its natural share of the electric fluid due to its susceptibility of receiving it, under such pressure; but

* Also in my Elementary Lectures on Electricity, Octavo, published in the present year.

when the plates are brought into contact with each other, face to face, as represented in the figure, which is an edge view, it is very obvious that the electric pressure in the plane of contact has received a very material alteration; and a new distribution of the electric fluid takes place, in consequence of a momentary flow *from* the copper, through the plane of contact, *to* the zinc. The zinc having now received more of the fluid than it had whilst under the natural equable pressure, and the copper having lost that quantity, the former becomes *positively* and the latter *negatively* electrical. The distribution, however, is now of a peculiar kind, and very different to the distribution on each individual body prior to contact, which was equable on every side. The new distribution brings the pair of metals into an electro-polar state—the exterior surfaces of the zinc and copper being respectively positively and negatively electrical, not only with respect to each other, but with respect to the plane of contact, and also to exterior bodies whose natural circumambient electric pressure has not been disturbed.

14. If the metals be suddenly separated, whilst insulated, the redundant fluid on the zinc has not time to return to the copper: the former is, therefore, left in a positive and the latter in an electro-negative condition; but, if the separation be made slowly, the whole, or nearly the whole, of the redundant fluid will return to the copper plate, and the usual equilibriums on the two metals will be restored.

15. Now, since it is an invariable law in Electricity, that no flow of the electric fluid can take place from one body to another unless the former be positive to the latter, the experiment with Volta's discs furnishes us with a piece of interesting information, which, independently of some such experiment, we had no means of arriving at. It shows us in the most decisive manner, that *prior* to contact, and whilst the metals were subjected to the *same* electric pressure, the copper was positive to the zinc; but that, after contact, the zinc became positive to the copper.* By proceeding cautiously with similar experiments on different bodies, the relative natural electric states of an extensive series of them might easily be obtained: this interesting piece of information, however, remains a desideratum, with the exception of a very few insulated facts.

16. If, whilst the copper and zinc plates are in contact, a copper wire were to connect their outer surfaces, a return of a small portion of the electric fluid, from the zinc to the copper, would take place, and a new distribution, and subsequent equilibrium, would be the results. But as the transverse sectional area of the conducting arc, which rested on the surface of the zinc, would be extremely small when compared with the whole area of that surface, the new distribution of the fluid, produced by the application of the arc, would be very little different to that displayed *prior* to

* I am well aware that many philosophers are of opinion that zinc is *invariably* positive to copper, but I am not aware that any of them have given a reason for entertaining that opinion.

such application, and would not be productive of any change in the *general character* of the polarization.

17. If, however, the conducting arc were of large transverse dimensions, or if a great number of small ones were employed, so as to cover, by their sectional areas, a large portion of the surface of the zinc plate, the previous distribution, due to the contact of the copper plate, would be very much altered. Nevertheless, the display of polarization, although much lessened in *degree*, would still retain its original *character* on every portion of the exterior surface of the zinc that remained unoccupied by the ends of the conducting arcs; for it is not until the whole surface of the zinc is completely covered with copper, that the polarization ceases to be displayed.

18. I have made extensive series of experiments on this delicate part of the subject, and from their results I have been led to conclude, that these facts are not limited to the employment of copper and zinc only, but that they are producible, and can be satisfactorily displayed by any two metals whatever; and not only by the employment of two *distinct kinds* of metal, but by two discs of one individual metal, cast from one and the same fused mass, provided the surfaces be of different degrees of polish.

19, Now, when the polarization first takes place, by the simple contact of Volta's plates, and the new equilibrium has become established, it is obvious that the electric force on the zinc surface has become increased, and that on the copper surface diminished. Hence it is, that the respective electric tendencies of those surfaces, with reference to the circumambient medium, are very different—the zinc having a tendency to *dispose* of a portion of its fluid, and the copper a corresponding tendency to *receive* a portion. Under these circumstances we have an opportunity of either increasing or diminishing the sum total, or general stock, of the electric fluid in the two plates. If, for instance, I touch the copper plate with an uninsulated conductor, a new portion of fluid is transmitted to its previously negative surface, and the general stock is increased; but if, on the contrary, I touch the exterior surface of the zinc plate with a similarly situated conductor, a portion of the fluid, previously accumulated on that surface, is delivered over to the conductor, and the general stock becomes diminished. A moment's contact with the conductor, in each case, is sufficient to produce the effects stated, which may be proved by separating the plates slowly afterwards. In the former case both plates are found to be electro-positive, and in the latter case both are found to by electro-negative.*

20. The principles on which the display of the above described phenomena depend, may be conveniently taken advantage of in exhibiting the electric conditions of the plates by their simple contact; because the action of each individual plate is enhanced by applying a conductor to the other, whilst they are in contact. If, for instance,

* For the performance of these experiments the most delicate electroscope must be employed.

I wish to show the positively electric action of the zinc to the greatest advantage, I insulate that plate only, and not the copper; and, on the other hand, if I wish to show the negative electric action of the copper plate to advantage, I insulate it only, and keep my hand in contact with the zinc. By these means, a single contact is sufficient to show the electric character of each plate.

21. Nothing could be more satisfactory in establishing a general law in Electricity than the facts here stated: viz. that *by the simple contact of dissimilar metallic bodies, a partial transfer of the electric fluid from one to the other invariably takes place.* This is not only a general law in Electricity, but also one of the *fundamental laws* in *Voltaism*, or Voltaic Electricity, in all those associations in which two dissimilar metals are employed: and the same law is applicable in all other Voltaic associations, whatever may be the character of the materials which enter into them.

22. Therefore, the first step in every case of Voltaic electric action is a transfer of electric fluid from one of the bodies to another. This first move of the electric fluid gives rise to a new electro-distribution—to electro-polarization; and, in those cases where the group is *insulated*, to a new electro-equilibrium of stability.

23. Having thus satisfied ourselves respecting the primary and secondary electric conditions of each individual pair of metals, our next consideration is to ascertain what takes place in a *series* of pairs placed within the spheres of each other's action—the simplest of which is that of the dry pile.

24. When two pairs, A and B, Fig. 2, are placed in such a manner, with respect to each other, that the positive surface of A is directly opposite the negative surface of B, having only a thin film of air between them, the previous electro-equilibrium of each pair will again be disturbed; for the accumulated fluid on the *inner* or positive surface of A, will charge the thin film of air, and thus cause it to exert a greater electric pressure on the vicinal *negative* surface of B, than that to which it was previously exposed; and no corresponding pressure taking place on the exterior or positive surface of this latter pair, its fluid will be urged in that direction, and consequently the accumulation of fluid on that surface of B will be increased. The disturbance of the fluid in A arises from the electric pressure on its inner or positive surface being diminished by the vicinal negative surface of B, without any corresponding diminution of pressure on its exterior surface, the first effect of which is a movement of the fluid in A towards B. It, therefore, appears that the fluid in both pairs moves in one and the same direction, and that the ultimity is a new equilibrium in the group, in which a more powerful electro-polarity is established than can be displayed by either pair alone.

25. It will now appear very obvious, that if a third pair, C, were to be added to A and B, the electro-polarity at the extremities of the series would become still greater than that displayed by a group of two pairs only; and, for the same reason, every additional pair would cause an increase of polarity in the series, which, when extended to

about 100 pairs, would be sufficiently powerful to affect electroscopes, and put light pendulous bodies into motion.

26. The electro-polarization of bodies may be enhanced either by augmenting the disturbing force, or by lessening the resistance of the surrounding medium. The latter circumstance is usually resorted to in the construction of the electric column, in which discs of dry paper, instead of films of air, form the interposing medium to the metallic pairs.

27. M. Marechaux was the first philosopher who employed paper in the dry electric column. The metals in M. de Luc's columns were discs of thin zinc, and of Dutch gilt paper—the gilt side of the paper being in contact with the zinc in every pair. The series was strung upon a silken thread, which passed through the centre of the whole, and then placed in a glass tube furnished with brass caps and ball at its extremities, as represented by Fig. 3, Plate VIII.

28. When one extremity of the pile is held in the hand, and the other placed on the cap of an electroscope, the gold leaves immediately diverge, indicating the electric character of the pole in contact with the instrument. Or the column may be placed horizontally on the caps of two gold-leaf electroscopes, as represented by Fig. 4. In this case both instruments indicate electric action in the extremities of the column—the one positive and the other negative, the zinc end invariably displaying the *electro-positive*. By a series of 20,000 pairs, the late Mr. Singer was enabled to charge Leyden jars by a moment's contact. By the employment of coated talc, instead of glass, I find that an extensive surface may be charged by a dry pile of 10,000 pairs.

29. The principal consideration in this place, is the perpetuity of the action of the dry electric column, which, when properly constructed, and securely guarded against moisture, appears to be possessed of interminable electric powers. At the time when philosophers, especially the chemical part of them, could form no idea of the changes in the electric characters of bodies but such as emanated from *oxidation*, the most fruitful of all sources of error in this branch of physics, the dry electric column shared the same fate as the wet pile of Volta, in having its action placed to the credit of oxidation. And even when it was found that the dry column would retain its action for several years, a *slow* oxidation of the metals was the only explanation that could be given for the display of its Electricity. This idea became so fashionable eventually, that it formed the basis of the prevailing hypothesis, which, to this day, is strongly contended for by certain philosophers now in controversy on this interesting topic.

30. To those who have paid attention to the succession of steps that I have hitherto taken, in tracing the electric action from metal to metal in each individual pair, and also that action which is due to a series of pairs, as associated in the dry electric column, there can appear no reason whatever for calling into the hopothesis

an oxidation of the metals; but, on the contrary, since purity of the metallic surfaces is essential to the development of those peculiar arrangments of the electric forces which I have described, and which form the very soul of the dry electric column; and since the introduction of an oxidizing process would soon polute these metallic surfaces, and ultimately destroy every particle of their metallic character, nothing can appear more evident in physical science than that *a perpetuity of electric action in the dry pile depends upon a continuance of purity in the metals.*

31. The inference thus drawn from the simple principles of Electricity, is that alone which can direct the *practical* Electrician with certainty to the construction of a *permanently* acting electric column. It is conformable with all experience, and I believe is now acted upon by those who make the best apparatus of this kind. Every care is taken to insulate the elements of the column from the atmospheric air, and from every kind of moisture, which, when properly accomplished, the permanency of electric action becomes secured.

32. The dry electric piles of Zamboni are those which have been the most securely protected against the action of the ambient air; and are those alone which have maintained their original electrical intensity. They are formed of discs of thin zinc foil, covered on one side with black oxide of manganese, which answers the purpose of a second metal. The manganese being reduced to an impalpable powder, is mixed with honey, and laid on the metal, like a varnish, whilst in this state. The whole of the liquid, and every vestage of moisture, is afterwards dissipated by the heat of an oven, and the oxide left quite dry on the metal. About a thousand of these discs, with corresponding discs of writing paper, also quite dry, intervening, are formed into a column, with the blackened surfaces in one uniform direction, and consequently the metallic surfaces in the opposite direction; thus arranged the elements of the column are squeezed closely together, by means of four silken cords or braces, joining the extremities of the group at equal distances from each other. Whilst in this condition the column is dipped into melted sulphur, which, when solidified by cooling, forms an insulating case; the extremities or poles alone being left bare, the appartus is complete.

33. The electric pile alluded to in the note at page 147 consists of a series of 600 square pieces of thin rolled zinc, with intervening pieces of dry writing paper of a similar size and shape. They are arranged in four groups—one in each of the four compartments, into which a shallow square box is divided by glass partitions. The box is of wood, lined with glass. The metals were cut from sheet zinc, which had long been in the capacity of a water-spout, consequently corroded by exposure to the air and water. One side of each piece was made quite bright and smooth, by means of a file, and the other side rough and covered with the grey oxide. Fig. 11, Plate VIII. is a representation of the apparatus without its cover, and exposes the edges

of the metallic pieces, which are arranged in such a manner that the letter *b* represents that end of each compartment towards which all the *bright surfaces* within it are placed; and the end *d* of each compartment is that towards which the *dull surfaces* are placed. Two plies of writing paper are placed alternately with the metals, and the elements of each group are squeezed compactly together by means of small wedges introduced at the ends of the compartments. I have found, by extensive trials, that the closer the elements of any dry pile are pressed together, the better the action of the apparatus. The several groups are united by means of staples made of brass wire, which reach from *d* of the first group to *b* of the second, and so on from the second to the third, and from that again to the fourth; so that *b* of the first group, and *d* of the fourth, are the poles of the whole series. This arrangement acted very well on the gold-leaf electroscope for five or six years; but not being protected from the atmosphere, its action gradually languished, and eventually became extinct for reasons already stated (30). Also fifth Memoir (3).

34. Besides the arrangement last described, I formed others about the same time (1827) with bright zinc and dry writing paper. The metallic pieces were made rough on one side by means of a rough file—the opposite surface being finely polished. In these arrangements the smooth surfaces were electro-positive to the rough ones; and the poles displayed electric action as promptly as with any other form of piles, though by no means so powerful as that last described (33).

35. As the whole action of the column depends upon electric pressures, any change in the external electric pressure of the atmosphere will necessarily affect the action of the apparatus. It is on this account that it becomes an indicator of those fluctions of atmospheric electrical pressure which are almost continually going on; and as these fluctuations are occasioned by change of temperature, humidity, evaporation, winds, barometric pressures, clouds, &c., M. de Luc employed the electric column as a meteorological instrument. The frequent changes in the activity of this apparatus, first noticed by that ingenious and indefatigable philosophèr, are exceedingly curious and interesting. He attached to the upper pole of a vertical electric column, a bent wire, which reached downwards as low as the lower pole, and terminated in a small brass ball. Between this ball and the lower pole of the column was suspended, by a silken fibre, a light gilt pith ball; the whole being covered by a glass shade, as represented in Fig. 5. The pendulous ball vibrates between the lower pole of the column and the opposite ball, carrying the electric fluid from one to the other continually, thus producing a pulsatory current in the pile. When the atmosphere is highly charged with the electric fluid, the ball vibrates with great rapidity; it also moves rapidly in a warm room, or any warm unattenuated atmosphere, but its activity languishes very materially in a moist atmosphere.

36. When two columns are arranged, as in Fig. 6, having a bell attached to each lower pole (the one positive and the other negative), the pendulous brass ball plays between them, and rings both bells—thus warning the observer when any material atmospheric electrical change is taking place. Apparatus of this kind have kept in play for upwards of twenty years, and are still as active as at first; and there can be no satisfactory reason shown why they should ever cease to display their electric forces.

37. Having now disposed of the views which I have taken of the theory of the dry electric column, I shall next endeavour to explain the principles upon which the wet pile, or Voltaic battery, operates; and why its action soon languishes, and ultimately becomes annihilated. And in this attempt, as in that of the previous part of the subject, I shall proceed with the simplest case.

38. The simplest case in the action of the wet pile of Volta is similar to that of the dry pile already discussed; and the action of such a pile, under certain circumstances, would be as durable as that of any dry pile whatever, provided the electric condition of each individual metallic surface were uniform; but in consequence of an ununiformity of electric action on various points of every metallic surface, as already explained in the first part of the sixth Memoir, any moisture in contact with such surface would suffer partial decomposition, and the metal itself undergo a change, whether employed as an element in a Voltaic series or not.

39. When, however, pure water is employed for the intervening medium between the metallic pairs, the pile assumes electro-polarity in a supereminent degree; and a series of about 500 pairs would charge an extensive coated surface of glass, by one momentary contact of either of its poles. 500 square feet of coated glass have been charged, by a pile of this description, to a higher degree of intensity than can possibly be accomplished in the same time by the most powerful electric machine; and by increasing the extent of the Voltaic series, the charge would increase in proportion. Hence, from a source of this kind, Electricity, to almost any required extent, might be obtained; and although the local electric action, especially on the surface of the zinc, tends continually to lessen the general electric forces of the pile, and eventually annihilates them altogether, the apparatus retains a considerable degree of action for many successive hours. The theory of the general action of this apparatus is precisely that already given for the dry electric column; and it is a matter of no consequence whether the metals be piled one upon another, with intervening moistened paper, as represented by Fig. 7, or that they be placed edge-ways, in a "Cruickshank's trough," represented by Fig. 8, Plate VIII. whose cells are newly filled with pure water. In this latter form, however, the cells must be perfectly water-tight, and the pairs of metal well insulated from each other by resinous cement. A pendulous ball, suspended between the poles of a water-charged battery, would produce *pulsatory* electric currents upon the same principle as with the dry pile.

40. When the Voltaic pile or battery is employed in its most usual capacity for the production of electric currents, the principles hitherto described form a part only of the theory of its action; for *stable* electro-equilibrium is inconsistent with the idea of a continuous flowing current; therefore, other principles must enter the framework of the theory, in order that its explanations may become satisfactorily applicable to every variety of case.

41. When a pair of copper and zinc plates are united by wire, as in Fig. 9, the pair becomes electro-polar as decidedly as in any other case; but not so powerfully so as when the planes of the plates are laid parallel to each other, as in Fig. 1. They, however, still form a Voltaic pair, and the zinc receives a portion of the electric fluid previously belonging to the copper, and, consequently, is in a suitable condition to give up that portion to any conducting body capable of receiving it. The copper plate also, being now in a negative state, is equally prepared to receive a new portion of fluid from any body appropriately situated to communicate it.

42. Let us now suppose that the metals are immersed in water, which is a compound body, and whose particles are susceptible of motion by the application of slight forces, and of separation from one another by the introduction of other matter amongst them; also that the electro-conduction of water is improveable by admixture with acids or alkaline matter, &c., and its constituents held together by certain electric forces, and, consequently, susceptible of separation by electric forces of superior power. Under these circumstances, the first immersion of the plates into pure water would cause a movement of their electric fluid, in such a manner that the zinc would give up a portion to the water, and the copper would receive a portion from it; and, if nothing further went on, there would be a new distribution of the electric fluid, and again a statical polar equilibrium established as decidedly as under any other circumstances.

43. With copper, zinc, and water, however, the electric forces of the metals are somewhat more powerful than those which unite the oxygen and hydrogen in the shape of water; and as the particles of water themselves are electro-polar, those of them next to the plates become easily arranged in regular polar order, with respect to the electric forces of those plates. The positive zinc surface attracts and draws towards it the negative surfaces of the particles of water; and the positive surfaces of another stratum of water become placed in juxta-position with the negative surface of the copper. The electric forces both of the metals and the water being now arranged in the best possible manner to accomplish a separation of the constituents of the latter, and subsequently urge them in opposite directions, accordingly to their relative electric characters, the combined forces, thus arrayed, vanquish those by which the constituents of the water were held together, conveying the hydrogen to the copper and the oxygen to the zinc; and during these motions of the constituents of the

water there would be a contemporaneous movement of the electric fluid, which, through the connecting wire, w, would be in the direction of the large arrow, and in the liquid in the direction of the small darts.

44. Now, since we are unable to discover, by observation, at what part of the water its decomposition takes place, the theory is necessarily left in some degree of obscurity on this particular point, which has given rise to much inconclusive discussion, and to opinions of many diverse kinds. In philosophical reasoning, analogy, in the absence of phenomena, often becomes a valuable substitute; and data thus supplied have led to inferences as satisfactory as if drawn from facts themselves: in the present case, however, analogy seems to be productive of various conclusions, nearly all of which are equally supported. It is known, for instance, that alkaline and acid matter, though placed in separate vessels, are made to traverse a Voltaic circuit, *exterior* to the battery, in opposite directions, even through each other—to exchange places, and occupy each other's positions in the two vessels. If this fact were to be made the basis of analogical reasoning, it would equally support either of two opinions in the decomposition of water by Voltaic electrical agency of a single pair, as in Fig. 9, for the action exterior to any battery is similar to that within it. It would be as applicable to the supposition of the decomposition taking place at the centre of the mass of water, between the copper and zinc within the battery, as to the supposition that decomposition occurs at both plates at the same time, transporting the constituents in opposite directions, in both cases. But there are other circumstances, yet to be noticed, which would lead to the inference that the *initial* effects take place at the metallic surfaces.

45. It has long appeared to me, that since all the metals and carbon, which are the best conductors of Electricity known, are invariably carried to the negative pole of the battery, or in the direction of the current through the liquid part of the circuit, there is something like a general tendency for the electric fluid to take possession of the best conductors in the liquid mass, and carry them to the next solid conductor; as, for instance, to the copper in a single pair. Should this be the case, it would be an easy matter to explain the reason of alkaline matter being invariably determined at the negative metal; for the potassium, or other alkaline metallic base, would arrive there as a pure metal; but, being re-oxidized as fast as liberated, it would re-assume the alkaline state and dissolve in the liquid. The same reasoning also applies to strontia and other compound bodies which are known to arrive at the negative metal, when, in connection with other matter, they are submitted to the action of an electric current.

46. It is now some years since I attempted to show the correctness of this view, in the *Philosophical Magazine*,* and I have not yet met with any fact that has tended to militate against it. I there showed that when a mixed solution of two metallic salts,

* See Supplement to the sixth Memoir, page 170.

the sulphate of copper and the sulphate of zinc, is subjected to the action of an electric current, the copper, which is a better conductor than zinc, is carried *alone* to the negative polar terminal; and that, by this means, a considerable portion of copper can be separated from the zinc. From this fact I had every reason to suppose that, if any of the metals be compound bodies, as some have thought them to be, their decomposition might be effected according to the same law, and by a similar mode of operation. This hint was thrown out in the same paper. I have, however, found that the results of all experiments of this kind are modified by the strength of the metallic solutions and that of the battery employed; but the fact that *all the metals* (which constitute about three-fourths of the known simple bodies) being determined at the negative polar terminal, when liberated from their combinations with other bodies by electric currents, is strongly in favour of the views I have taken on this topic.

47. By an experiment of more modern date than that already alluded to (46), and one never till now made public, we have strong reasons for supposing that the alkaline metals are transported to, and liberated at, the *negative terminal*, in a pure metallic state; and also that the *initial* point of decomposition, exterior to the battery, is close to the terminals. By operating on a strong solution of any of the salts of potassium (nitrate of potash, for instance), the metallic base can be liberated by the following process:—Immerse the *positive platinum terminal* in the solution; afterwards just dip the point of the negative platinum terminal (a wire is best) into the liquid, and that moment the potassium is liberated and burns with the usual coloured light on the surface. This phenomenon is constantly, and may be *continuously* displayed for a long time without interruption, whatever may be the distance between the terminal metals, provided the battery be of sufficient power.

48. There is another interesting fact, not noticed in works on Voltaic Electricity, which bears strictly on this topic, and at the same time develops a peculiar law in this department of physics. If the terminal platinum wires of the battery be placed horizontally in a portion of water, with their extremities pointing to each other, as represented by Fig. 10, Plate VIII. it will be found, as soon as the circuit is completed, that the decomposition of the water commences at the extreme points, P and N, of the wires, and slowly advances on both of them, with a gradually diminishing rapidity, until it has arrived at a distant part of each, where the action is too feeble to produce any decomposition whatever. This is a remarkably beautiful experiment, and highly illustrative of the distribution of the forces in the terminal or polar wires; that they are the most formidable at the extremities most remote from the battery, and gradually diminish in intensity, along those wires, until their effects are entirely lost. Fig. 12, Plate VIII. will afford a good idea of the proportions of gas liberated by different parts of each wire, the dotted parts representing the ascent of the gases.

49. In all cases where the terminal wires enter the fluid to be decomposed, the action commences at the extremities most remote from the battery; but in no experiment that I am acquainted with is the distribution of the decomposing force so clearly manifested as by that last described (48).

50. The above described facts manifest an electro-polar action in the metalic portions of the circuit, but there are others which as decidedly demonstrate an extention of that action into the liquid operated on between them. In the decomposition of a solution of the sulphate of potash, in litmus liquor, the liberated acid and alkali first indicate their presence at the points of the wires; but eventually one half of the liquid, between the wires, becomes red, and the other half green; maintaining their respective positions on each side of the plain of demarcation, as if separated by a solid barrier. Hence we learn, that the liquid, between the terminal wires, is as decidedly electro-polar as the wires themselves; and as the action *within* the battery is similar to that displayed exterior to it, there appears indubitable evidence of electro-polarity in the whole of its elements, both solid and fluid, and that its *primary* action is due to electric forces.

51. The term "*electric current*" has become so exceedingly familiar and convenient, that it is employed without hesitancy by almost every writer on the subject, though the universal taciturnity on the mode of propagation leaves a sad blank in every hypothesis hitherto made public.* The principles I have already embodied in this Memoir, although perfectly explanatory of the preliminary conditions essential to the production of a Voltaic electric current, as far as electro-distribution and polarization are concerned, would leave the associated group of metals and water in a perfect electro-statical repose. But it has been shown (43) that such an equilibrium is not stable with such an arrangement of these materials, and that the first consequence is a decomposition of a portion of the water, which, once accomplished, motion is again produced in the electric fluid; and so long as any part of the water is suffering a change from its constituents being torn asunder by the assailing electric forces, no stable equilibrium can be maintained, and the electric fluid, as a matter of course, is kept in a continual state of commotion. Hence it is, that the decomposition itself, which is the primary effect of the combined electric forces of the metals and the water, is not only the *first step* in the production of a current, but absolutely essential to its existence and propagation.

52. Volta, whose theory of the pile was similar to that I have explained, with the exception of the view I have taken respecting the propagation of the current (51), considered that the water was a mere conductor between the metals; but it is easily demonstrated that *no electric current, nor electro-circulation, can possibly be produced*

* In a small work on "*Electro-Gilding, &c.*" lately published, and occasionally in other places, I have attempted to show by what means Voltaic electric currents are propagated.

independently of motion in some of the other elements employed in the arrangement. I have shown, some years ago, in the *Philosophical Magazine*, and I think in the *Annals of Electricity* also, that in the production of thermo-electric currents, where the *calorific* matter is the *motive agent*, and in the production of magnetic electric currents, where the *magnetic* matter is the *motive agent*, a disturbance of those agents is an essential preliminary to the existence of these currents, and an indispensable circumstance in their propagation. And it is equally demonstrable that, with whatever degree of force the electric fluid might be disposed to expand and move in any one direction more than another, it would speedily equilibrate amongst such bodies as would not yield to its powers, and again assume a determinate statical repose. Hence, under such circumstances, no propagation of a current could possibly take place.*

53. By some writers on this subject, the existence even of a current has been questioned, but these are few in number, and those few have advanced nothing to the contrary but mere sceptical surmise, probably more with a view of appearing singular than from any train of reasoning they have bestowed on the subject. That a current does exist no experienced man can doubt; but, unfortunately for their readers, few writers are experienced men. We have some beautiful analogies in mechanical and Voltaic Electricity, and some peculiar phenomena exhibited in the latter branch, from whose direct inferences there seems to be no appeal.

54. If a series of pointed metallic wires were to be arranged, as in Fig. 13, and one of the extreme points placed near to the conductor of an electric machine in good action, a luminous star would tip every point in the series that is directed towards the prime conductor; and those points of the wires which are presented in the opposite direction, or *from* the conductor, would exhibit a beautiful brush of electrical light. Now, since it is admitted that the star and brush are respectively indicative of a *receiving* and *delivering* extremity in each wire, it follows that an electric current is passing through the whole series, and also through the interposed plates of air. Moreover, it must be born in mind that a *delivering* surface must be positive to that which *receives* the electric fluid from it. Hence we are led to understand that each wire and each plate of air are electro-polar, and that this polarity is essential to the existence of the current, and *vice versa*, the current is a consequence of the polarized state of the wires and interposed plates of air.

55. The counterpart of the last described phenomena is beautifully displayed in Voltaic Electricity. Let a series of metallic wires be arranged in a glass tube holding acidulated water, as represented in Fig. 14, and the two outermost ends of the extreme wires in connection with the two poles of a Voltaic battery: every wire in the series becomes electro-polar, liberating hydrogen at those extremities which point towards the *positive* pole, and oxygen at those which point in the opposite direction.

* See twenty-second Memoir (112.)

If the tube contain a blue neutral solution of acid and alkaline matter, and the wires be platinum or gold, the well known characteristic red and green colours will shortly appear. Now, since there is an electric current produced in the former case (54), the analogy is sufficiently striking and complete to infer that a current is also in existence in the latter (55). Indeed no other satisfactory conclusion can be arrived at, and since a series of wires, similarly arranged *within* the liquid in a battery, display phenomena as decidedly as a series *exterior* to it (in the glass tube, for instance) there can be no reason to doubt the existence of a current throughout the whole system.

56. The Electro-Chemical action *within* the battery is beautifully illustrated by suspending short platinum wires within the liquid between the Voltaic plates. If, for instance, the darts, *n n n*, in Fig. 9, Plate VIII. represent three platinum wires suspended in a solution of sulphate of copper between the Voltaic plates C and Z, the barbed ends will become *electro-positive* and liberate oxygen, and the ends, *n n n*, being *electro-negative*, attract metallic copper from the solution.

57. If P N, Fig. 15, Plate VIII. be permitted to represent an edge view of a series of Voltaic pairs, arranged as in the Cruickshank's battery, charged with sulphate of copper; and B B B a trough, also holding a solution of sulphate of copper, united to the battery by the conducting wires, W W, and the platinum plates, P' N'; the whole of the wires, *p n*, *p n*, *p n*, *within* the battery, and also the wire, *p n*, and the two plates, *p n*, *p n*, in the trough, B B B, will display electro-polarity, and liberate oxygen at the surfaces, *p p p*, &c. and become covered with copper on the opposite surfaces of the plates and points of the wires, *n n n*, &c. throughout the series. The plates are suspended in the trough, with their flat surfaces at right angles to the direction of the current: they may be of the thinest leaves of silver or platinum, and the opposite surfaces will become electro-polar as promptly as if of thicker dimensions—a fact corroborative of the views I have taken respecting the electric condition of the thin films constituting metallic crystals.* (Second Memoir, page 82.)

58. The direct effects of an electric current are strikingly manifested by the different appearances of charcoal points, after deflagration—the *delivering* piece being pointed, and the *receiving* piece being rendered concave.

59. It is well known that I have for many years advocated the *non-identity* of the electric fluid and the calorific fluid—(twenty-second Memoir, 28);† and I know of no fact that supports these views more completely than that which I discovered in the autumn of 1838, with a battery of one hundred and sixty pairs of copper and zinc.‡ In that remarkable phenomenon I can see no other mode of explanation than that

* A complete series of experimental illustrations will be found in my work on "*Galvanism*," in twelve Elementary Lectures.

† Annals of Electricity, &c. vol ii. page 416.

‡ The particulars of this battery, and of the experiments in question, will be found in the Annals of Electricity, vol. v. page 365; also in Silliman's American Journal. In one of my lectures at this Institution, in the present month (May), I produced the same phenomenon by fifty pairs of Grove's battery.

which I have already given in my letter to Professor Silliman,* viz.—that the electric fluid, which is an exceedingly active agent, was enabled by its volant powers to spring from wire to wire through the intermediate stratum of air: whilst the comparatively sluggish calorific fluid, being unable to traverse the aerial space with the same degree of velocity, was thrust *out* of the electric path, and forcibly compressed into the remotest extremity of the *positive* wire, and entirely exterior to the electric circuit.

60, But the same phenomenon, which is so happily applicable to the support of those views (59) is also demonstrative of the existence of a current; and perfectly conclusive of the direction in which it flows through the conducting wires for reasons already given. Moreover, the direction of the current in a Voltaic circuit is demonstrable by its magnetic effects, from the analogies which they exhibit with those manifested by the discharge of a Leyden jar. Since, therefore, the existence of electric currents in a Voltaic circuit is so strikingly manifested by the display of so many indubitable facts, those currents necessarily emanate from a primary cause; which, as I have already stated (52), is not to be found in any group of unyielding and unalterable bodies: and, as the metals suffer no alteration prior to the decomposition of the water, this latter is the primary effect of the electro-polarization, and is essential to the production and propagation of the current.

61. The facts here stated, in connection with those described in variovs parts of the fifth Memoir, appear to form a sufficient guide to direct our views to the true character of those primitive forces which actuate Voltaic batteries in the production of their various classes of phenomena. The hypothesis of Wollaston, which rested on the supposition of chemical agency (especially on one of the metals employed), has no longer a claim on the attention of philosophers (fifth Memoir, from page 129 to 137) for, although the developement of chemical phenomena *within* the battery is an essential circumstance in the propagation of its electric currents (52), those phenomena can be regarded in no other point of view than as the offspring of electric forces.

52. The hypothesis of Volta, although justly based on primitive electric action, makes no provision for the propagation of the phenomena of currents. Moreover, the *indispensibleness* of immediate contact of the metals employed, being also deemed an essential element in the theoretical views of that eminent philosopher, is a serious error embraced in his hypothesis. (Fifth Memoir (9), page 126, and (86-92), page 155). When a pair of metals are united by means of a metallic wire, as represented by Fig. 9, Plate VIII. the channel of conduction is more copious and better adapted for the display of a current than if united by a liquid, as in the experiments with the *unclosed Galvanometer* (pages 154 and 155). But the production of a current, though

* This letter forms one of the Miscellaneous Articles in the present volume (Section VI.)

not to the same extent of force, is as certain by the latter arrangement as by the former. This important fact, so essentially connected and interwoven with the true theory, is a manifest demonstration of the inaccuracy of that part of Volta's hypothesis which requires immediate contact of the metallic plates.*

63. Nor, indeed, was it to be expected that Volta, whose hypothesis was framed in the infancy of the science, could be furnished with all the necessary materials for building a true theory. And, even now, although we have many additional facts at command—developements of comparatively recent date, and unknown to the celebrated philosopher of Como,—it is possible that some essential part of the fabric may yet remain undiscovered; and which, at some future day, will claim its proper place in the true theoretic structure.

64. The influence of asperous surfaces, the changes of electrical character which metallic bodies display by compression, and the extent of *local* action or their surfaces (see fifth and sixth Memoirs), have all become essential elements in the theory of Voltaic Electricity. The liquids also, as well as the solid parts of the Voltaic arrangements, display an immense influence in producing electrical commotions, and must always take a prominent stand amongst the topics that engage the attention of the electrical philosopher.

65. The complete and entire exclusion of chemical action, in the best operating dry piles, leaves the electric agency in full possession of every view that can be taken respecting the character of their actuating powers. In the wet pile also, were it possible to prevent electro-chemical decomposition, electro-statical phenomena would be as promptly and permanently displayed as in the dry pile of Zamboni; and in both cases *pulsatory* currents, by means of pendulous bodies, might be propagated for any required period of time.

66. In acid-charged batteries, however, pendulous messengers are not required to transport the electric agent from pole to pole: the joint electric forces of the solids and fluids soon liberate constituent elements within the group, and convert them into vehicles for the maintenance of circulation and the display of phenomena in the circuit of conduction.

W. S.

Royal Victoria Gallery of Practical Science,
Manchester, June, 1842.

* I hope to have committed no error in styling this an "important fact," seeing that it has been deemed sufficiently so to become the principal theme of Dr. Faraday's *Eighth Series of Researches*, dated 1834, or four years subsequently to its original announcement in my pamphlet. It is a remarkable fact also, that my discoveries of the superiority of *rolled* over *cast* zinc in conferring action on Voltaic batteries, and also of the advantage which *new* zinc plates have over old ones, &c., should be placed amongst the principal topics (as original developments) in Dr. Faraday's *Tenth Series*, or five years subsequently to the publication of my pamphlet, in which the whole investigation is clearly described. (See fifth Memoirs, from page 144 to page 151.)

ON THE DISTRIBUTION AND RETENTION OF MAGNETIC POLARITY IN METALLIC BODIES.

NINTH MEMOIR.

The retention of magnetic polarity in hardened steel is so very strikingly manifested in artificial magnets of various forms, and the circumstance is so well known that it would be quite unnecessary in this place to advance any remarks concerning it, further than merely to name it for form's sake in the order in which it stands, and the pre-eminent rank which it universally holds amongst the metals as to the display of this mysterious faculty.

The retention and distribution of magnetic polarity in other metals, however, are by no means of such general notoriety; peculiar modes of experimenting are required for their exhibition, and but very few of their phenomena have yet had recognition beyond the sweep of the curious eye of the philosopher.

In the fourth Memoir I had occasion to allude to the retention of magnetic polarity (or the residual polarity, as I there called it) displayed in soft iron, which had previously been excited to an exalted degree of intensity. My remarks in that communication, however, were confined to soft iron in bars, in the various forms in which it is usually employed as electro-magnets.* In the present my observations will be extended to the retention, and also to the distribution, of magnetic polarity in thin sheet iron and other metallic bodies, when in the shape of discs.

The distribution and retention of magnetic polarity in thin discs of soft iron by rotation, when placed in various positions with reference to the magnetic meridian, &c. were first noticed and investigated by Professor Christie, in an extensive series of highly interesting experiments, which were published in the *Philosophical Transactions*, for 1825.† Mr. Christie very politely showed me several of his experiments at the time he was carrying them on, and, when published, presented me with a copy of the paper in which they were described. In a very short time afterwards I repeated some of those experiments with a temporary apparatus, which I fitted up for the purpose, and obtained the most satisfactory and corroborative results. Besides repeating the original experiments, however, I was led by curiosity to institute others, which, as some of the result are rather curious, I now propose to detail.

* See pages 117 and 118.

† Mr. Christie's paper was read before the Royal Society, May 12, 1825.

Experiment 1.—Let *a b c d*, Fig. 1, Plate IX. represent a disc of sheet iron, about fifteen inches in diameter, placed south of, and in the same horizontal plane as the needle *n s*, with its centre in the magnetic meridian, and a neutral point, *a*, in its northern edge, about one inch distant from the pole *n*.

If the north* pole of a powerful bar magnet, having its axis vertical, be now passed over a diameter of the disc from *c* to *a*, as indicated by the arrow, and about half an inch above it,† and taken away suddenly from the point, *a*, the plate will exhibit south polarity at that point; and the pole, *n*, of the needle will be attracted with a considerable force. If, now, a small delicate dipping needle be carried gently over the surface of the plate, feeble poles of both characters will sometimes be found in various parts of the iron, varying in power and situation almost every time the magnet repeats the excitation; but in no case will a north pole be found of equal vigour with the south pole determined at the point, *a*. In several instances I have found no regular pole only at *a*—all the rest of the plate exhibiting a diffused polarity without any apparent central pole. It sometimes happens, however, that all the south polarity of the plate is separated from its north polarity by an irregular curve line, as represented by the dotted line, *e q*, Fig. 1, which line may be regarded as a magnetic equator—every part of the plate on N side of that line being north polar, and all that part on S side of the line, *e q*, being south polar, and S the situation of the principal pole.

Experiment 2.—Let the north pole of the magnet be carried once round, and just within the edge of the disc, at the same distance above it as before, and in the direction of the arrows, Fig. 2, commencing and terminating the revolution at the point *a*, and suddenly quitting it as before.

The plate will again exhibit polarity; but the north pole of the needle, when reconciled, will be deflected towards the *west*, as the dotted position, *n' s'*, Fig. 2, showing that the attracting pole is situated in the limb, *a d*.

Experiment 3.—Let the same pole of the magnet be now carried round the plate in the opposite direction, from the point, *a*, to the same point again, quitting *a* as in the last experiment. The north pole of the needle will now repose towards the *east* of the magnetic meridian, indicating the attracting pole to be in the limb, *a b*, Fig. 3.

It appears by the phenomena exhibited in the last two experiments that there is a peculiar distribution of magnetic polarity by passing the pole of the magnet round the edge of the disc, differing considerably from that occasioned by passing the pole over a diameter. There is, however, as decided a regularity in the distribution of

* That pole of the needle which is solicited by the southern parts of the earth is here called north.

† A round piece of board, of the same diameter as the iron plate, and of a proper thickness, I have found very convenient to prevent the magnet from touching the plate.

polarity by this process as by the former; for although the pole be not determined in the point, *a*, where the magnet quits the plate, its position is invariably found in *that limb* which was last passed over by the magnet. The other species of polarity is diffused over a great part of the area of the plate without any apparent determinate pole.

The north pole of the magnet, when first brought over the edge of the disc at the point, *a*, excites south polarity in the iron immediately beneath it—in which point the disc would remain polar, if the magnet were to be withdrawn without passing over any other part of it. When, however, the pole of the magnet is permitted to pass round the plate, the parts over which it passes will become polar in succession, and the point, *a*, will receive the terminal exciting impression.

Now, as every point near to the edge of the disc may be supposed to be equally disposed to retain polarity, it is evident that as the exciting pole proceeds in its revolution, the successive poles which it generates will have an equal tendency to a permanent habitation; but in the same gradual succession as the exciting pole abandons them, and calls forth others in its progress, the primogenial poles will become enfeebled, progressively vanish, and eventually give place to a display of polarity of the other kind—which polarity, however, for want of an exciting pole to collect and condense it, is found scattered promiscuously in the area of the plate.

This being understood, it is plain that the parts over which the exciting pole last passes will exhibit a series of polar points more powerful than any other which it had previously abandoned; and those near to *a* on the last excited limb, a more powerful series than any other. The series of polar energy will, therefore, be *subordinate* from some point near *a* towards *b* or *d*, Figs. 2 and 3, according to the direction in which the revolving pole proceeds round the disc. The needle will, therefore, not be under the immediate influence of any individual point in the iron, but will be operated on by the conjoint forces of the vicinal poles, the resultant of which is in the limb last excited; and the pole of the needle being of an opposite character, will be attracted towards this resultant or aggregate pole.

Experiment 4.—Let the south pole of the magnet be now carried round the plate from *a* to *a*, in the same manner as the north pole was employed in experiments 2 and 3, and in the direction of the arrows in Fig. 4: the needle will be much agitated, but will eventually repose with its north pole deflected towards the *east* of the magnetic meridian.

In this case the aggregate pole of the last excited limb, *d a*, being of the same character as the vicinal pole of the needle, the deflection is produced by *repulsion*. The needle is, therefore, driven from the last excited limb, *d a*, of the plate, instead of being attracted by it, as in the former cases, when the deflections were produced by attraction.

Five separate revolutions of the south pole in the direction of the arrows in Fig. 4 gave the following results:—

	Degrees.	
1st round, ...	35	Deflection of the north pole, n, eastward, or in the direction n'.
2nd round, ...	42	
3rd round, ...	33	
4th round, ...	50	
5th round, ...	40	
Mean, . . .	40	

When the revolutions of the magnet were performed in the contrary direction, the results were as below:—

	Degrees.	
1st round, ...	40	Deflection of the north pole westward.
2nd round, ...	50	
3rd round, ...	45	
4th round, ...	58	
5th round, ...	65	
Mean, . . .	49·6	

The opposite polarity is promiscuously scattered in the area of the plate.

Experiment 5.—In this experiment the magnet is laid upon a round board, above and parallel to the plate; it must also be something shorter than a diameter of the plate.

Let the magnet be placed over the diameter, *a c*, with its south pole at *a*. Now cause it to revolve on its centre until each pole has completed one revolution, and remove it quickly from the plate parallel to its last position.

By this process the iron plate will become decidedly and regularly polar; and when the revolution of the magnet is in the direction of the arrows, Fig. 5, an aggregate *north* pole will be determined in the limb, *d a*, and an aggregate *south* pole in the limb, *b c*. The centre of force in those limbs will frequently be found in one and the same diameter of the plate, at a short distance from its edge, and generally between 5° and 10° from the diameter, *a c*. These poles are easily discovered by holding a small dipping needle over the plate. The equator of the plate, which separates its two polar regions, is also found in this way. The needle remains horizontal over this line, which is frequently an exact diameter of the plate, at right angles to its magnetic axis. If the needle be held on either side of this line, one pole or the

other will dip according to the polar character of the iron beneath it. Fig. 5 will give a pretty exact representation of the distribution of polarity of the plate, by one revolution of the magnet. The dotted line, *e q*, is the equator or line over which the needle exhibits no dip. From this equatorial line the dip increases towards S and N, over which points it stands vertical, showing the point S to be the south pole, and N to be the north pole. The plate is, therefore, a very regularly polarized magnet; and if means be not employed to disturb it, this distribution of polarity will be retained by the iron for some considerable time.

If the magnet were to perform its revolutions in the opposite direction, the polarity would be distributed as represented by Fig. 6.

When the plate is of good iron, and well annealed, these phenomena are very uniformly exhibited; but if it has been much hammered without subsequent annealing, the equatorial line is found variously curved, and the poles are not exactly in one diameter. In most cases, however, if the experiments be dexterously performed, there will be found a tolerably regular distribution of magnetic polarity. The disc may be placed in any other position, either vertical or sloping, whilst the magnet revolves round it, and the same kind of distribution will be effected. In several cases I have found that one revolution of the magnet will excite a polar energy in the disc, which, at six inches distance from the pivot of the needle, would be a counterbalance to the Magnetism of the earth at this place.

Experiment 6.—In this experiment the disc is made to revolve on its own plane, on a vertical axis, by means of a multiplying wheel and band. Over the plate is a light stage, on which is placed a powerful bar magnet, with its centre over the centre of motion, and its poles reaching nearly to opposite edges of the plate.

In this experiment the distribution of polarity is very singular and curious, and the retentive faculty of the iron is beautifully displayed.

Let *s n*, Fig. 7, represent the magnet placed on the stage over the revolving plate, whilst proceeding in the direction of the arrows. In this case it will be found, by holding a dipping needle over the plate whilst in motion, that *north* polarity is distributed over every part of the plate on the N side of the magnet, and south polarity over the other half on S side of the magnet. When the velocity is considerable, the polarity excited by one pole of the magnet has not *time* to change its character before the same point of the plate arrives immediately at the opposite pole; so that the intensity of polarity is pretty equal in every part of the circle, excepting the two points under the poles of the exciting magnet, at which points the transitions of polarity are continually going on, with a celerity proportioned to the speed of the plate.

When the plate revolves in the contrary direction, the two halves of it on different sides of the magnet change their polar character, as represented by Fig. 8.

These versatilities of polarity have a very pleasing effect when a dipping needle is placed on each side of the magnet. The alternations of dip in the two needles are simultaneous, and as prompt as the mutations of motion in the ferruginous plate beneath them.

Experiment 7.—Place the north pole of a magnet directly under the edge of the iron plate, so as not to touch it: in this case every part of the plate appears to be possessed of north polarity, and the south pole of the dipping needle inclines towards every part of its surface. This, at first sight, seems rather curious, because a south pole ought to be exhibited near to the edge where the exciting north pole is placed; and indeed this is the fact, for if a slight motion be given to the plate, south polarity is easily detected. Whilst the plate is at rest, the south pole of it being stationary, and directly over the north pole of the magnet, and also of inferior force, it is overpowered by the exciting pole, and the needle obeys the influence of the predominating polar energy. No velocity, however, that I can give the plate, will produce a change in the character of a dip. The angle certainly varies by placing the needle in various positions with regard to the magnet, and by varying the course of the plate; but in no instance has the character of the dip been destroyed. The same thing takes place when another north pole of a magnet is placed over the former, and above the plate.

When the plate is rotated between the poles of a horse-shoe magnet, the distribution of polarity depends upon the nearest pole, and is regulated as if that pole alone were present. There are, however, some other curious facts to be observed in these experiments which are not easily described. They may, perhaps, be of the same character as those which appear in copper and zinc, &c. and which are more easily traced, in consequence of the polarity excited in those metals being distributed in a peculiar manner, less vigorous, and the needle consequently more manageable.

Experiments on Copper, Zinc, &c.—We are indebted to the successful investigations of M. Arago for directing us to an experimental demonstration of the presence of Magnetism in copper, zinc, and all those metallic bodies in which, till then, plausible conjecture had alone supplied all the knowledge we possessed respecting its existence.

The experiments of that celebrated philosopher were first known in London in the early part of the year 1825. They consisted of a judicious application of the *magnetic momentum* generated by the rapid motion given to those metals (copper, zinc, &c.) in which the magnetic force is too feeble for detection by the simple presentation of the most delicate needle. Plates of those metals were rotated with great velocity, in their own planes, on a vertical axis, while a magnetic needle was delicately suspended over them. The magnetic force of the metals, by this process, became exalted in proportion to the rapidity of their motion; and generated to a sufficient degree of

intensity to deflect the needle from its natural position, and even to cause it to perform complete revolutions in the direction of those given to the plate beneath it.*

The experiments of M. Arago were immediately repeated with a great deal of interest in this country, particularly by Messrs. Babbage and Herschel, by Mr. Christie, Mr. Barlow, Mr. March, and by myself; and variously modified according to the views of the respective experimenters. The experiments and observations of Messrs. Babbage and Herschel, and those of Mr. Christie, were published in the *Philosophical Transactions*, for 1825, and those of Mr. Barlow and Mr. Marsh in the *Edinburgh Journal of Science.*

I repeated M. Arago's experiments about the same time that they were repeated by those gentlemen, and the observations which I had made were intended for early publication; but whilst drawing up an account of the experiments, and diversifying them in various ways, I found reason not to publish them quite so hastily. Some of my experiments, however, were well known to several scientific gentlemen in London and its vicinity in 1825, and without my knowledge were published in the *Edinburgh Journal of Science*, vol. 15. Since that time I have frequently exhibited such of them as are calculated for the lecture table, and on that account they may not, perhaps, be quite so interesting to some readers as they otherwise would have been.

The novel phenomenon of a magnetic needle rotating on its pivot by simply placing it above a revolving plate of copper, had something in it so exceedingly fascinating, and presented so striking a similitude to the electro-magnetic rotations with which the minds of philosophers were at that time so familiarized, that for a while it seemed doubtful whether or not the revolving plate possessed electric properties. If not, another question presented itself: does rotation produce Magnetism? Another mode of solving the problem was—that the copper plate, like all ferruginous bodies, actuated the needle by what is frequently called *induced* Magnetism by the influence of the earth; and that the needle was put into motion by a rapid succession of transient magnetic poles induced in the plate. When, however, it was found that the revolutions of the needle became more decisive, and were performed with greater promptitude in proportion to its polar energies, and that rapid revolutions were accomplished in light copper discs delicately suspended over a revolving horse-shoe or other powerful magnet, it readily occurred that all the phenomena emanated from the action of the magnet employed exciting polarity in the metals under examination; and that those metals possessing some degree of retention of Magnetism, the poles excited in them,

* The earlier experiments of M. Arago were made by vibrating a magnetic needle in the centre of a *ring* or hoop of each of the metals examined. Mr. Barlow had discovered that brass possessed magnetic properties some years before M. Arago's experiments were known. (See Barlow's Essay on *Magnetic Attractions*, 2nd edition, 1823, page 17) Also many years ago Cavallo met with instances of Magnetism in brass.

although transient, would necessarily lag behind whenever the celerity of motion exceeded the decay of polarity. The poles thus excited being of an opposite character to those of the exciting magnet, they would reciprocally attract each other; consequently in whatever direction the one was made to revolve, the other would follow after it.

This, I believe, was the explanation generally advanced, and it is perhaps, to a certain extent, very correct; but from the peculiar mode of experimenting which I had devised and pursued, I had an opportunity of observing certain phenomena which I considered that hypothesis inadequate to explain. I was, however, for a long time perplexed with irregularities in their exhibition, and unable to reconcile them to any determinate law. Nor was it till I was pursuing my inquiries concerning the *Thermo-Magnetism of simple metals*, in 1830,* that I could trace the phenomena I had observed to anything like uniformity; but by pursuing a hint afforded by the discovery of the curious distribution of the force excited by heat which actuates the needle, on the flat surfaces of simple metals, I became enabled to trace a similar, though distinct, distribution of force in thin discs of copper, zinc, &c. when under the influence of powerful magnetic poles.

I was first prompted to inquire into the distribution of magnetic polarity in discs of thin copper by a strange discordance of results, which were obtained by vibrating it within the influence of magnetic poles variously arranged with regard to its surface. Fig. 9 represents an elevation of the apparatus which I employed, and which first showed the anomalies in question. B B is a rectangular mahogany board, on which are erected the two brass pillars, *p p*, one of which supports the horse-shoe magnet, M: the other carries a pair of parallel projecting arms on the extremities of which the axis of a copper disc, *c*, is supported. To the lower edge of the disc is attached a weight or bob, to give it a vibrating tendency, so that by this arrangement the disc can be made to vibrate freely in its own plane between the arms of the apparatus and the poles of the magnet. A quadrantal screen, *s e*, of thin brass is attached to the front arm, and consequently a quarter of the disc is hid behind it. When at rest, and in the position represented by Fig. 9, the quarter behind the screen is divided equally by a radial line drawn on its surface.

When an experiment is to be made with this instrument, the bob is first to be brought to the point, *o*, of the arms, where there is a contrivance for retaining it in that position, or releasing it at pleasure. When in that position, the index line, *l e*, appears from behind the screen. The first part of the experiment is made without

* My paper on the Thermo-Magnetism of simple metals was not published till July and August, 1831, in the *Philosophical Magazine* and *Annals of Philosophy* for those months; but, had it not been for certain impediments which happened to be thrown in the way (see note to second Memoir, page 94), it would have been published much sooner, and this Memoir, which was intended to follow immediately after the former, would have been published in an early part of last summer (in 1831).

the presence of the magnet, M: when the trigger is pulled, the bob falls from the point, *o*, and performs a certain number of vibrations before the total disappearance of the index line, *l e*. This number being noted, the bob is again brought to the drop at *o*. The magnet is now placed on the stage, with its poles close to the surface of the disc—(the figure represents the magnet in such a position that the disc may vibrate between its poles). The trigger is again pulled, and the vibrations are counted till the index line is again lost sight of behind the screen.

With a thin copper disc, eight inches in diameter, vibrating between the poles of a horse-shoe magnet, the mean of six trials without the magnet, and of six with the magnet, were as below:—

Experiment 8.—Without the magnet . . . . 160 vibrations.
With the magnet . . . . 60 vibrations.

Experiment 9.—With a single disc of zinc:—

Without the magnet . . . . 150 vibrations.
With the magnet . . . . 60 vibrations.

It would appear by these two experiments, that copper is more affected than zinc by the presence of the magnet. This, however, is not always the case, for I have frequently found discs of zinc more affected than copper; there is also a difference even in the same kind of metal: and since a great deal depends upon the power of the magnet, as well as upon the character of the metal, it is plain that in all experiments of this kind, especially when intended for compairing the results with different metals, the same magnet ought to be employed, and the experiments made whilst it had the same standard power. With a very powerful magnet, I have frequently reduced the number of vibrations from 150 to 30, and in some cases still lower.

Hearing that Messrs. Herschel and Babbage had obtained some curious results by cutting the revolving disc in several of its radii, I also made some experiments with a disc of copper similarly cut. Fig. 10, Plate IX. will represent the manner in which the disc was cut. An experiment was first made whilst the disc was whole, and the means of six trials were as below:—

Experiment 10.—Without the magnet . . . 150 vibrations.
With the magnet . . . . 45 vibrations.

The disc was now cut with a pair of scissors, as at No. 1, Fig. 10, and an experiment made; next at No. 2, and another experiment made; and so on till nine slits were cut in the disc, and in every case the vibrations were performed between the poles of a horse-shoe magnet, as shown in Fig. 9.

		Vibrations.	
Exp. 11.	With 1 slit	48	The slit sides of the disc vibrating between the magnetic poles
	With 2 slits	54	
	With 3 slits	55	
	With 4 slits	59	
	With 5 slits	65	
	With 6 slits	67	
	With 7 slits	70	
	With 8 slits	70	
	With 9 slits	70	

I also tried an annular disc of copper, the inner diameter of which was six inches, and the external diameter eight inches. This rim was cemented to a disc of paste-board, and vibrated between the poles of a horse-shoe magnet.

Experiment 12.—Without the magnet . . . . 178 vibrations.
With the magnet . . . . 140 vibrations.

The results of this experiment show that the centre parts of copper discs are very much concerned either in receiving or transmitting the magnetic impressions; for those impressions, in whatever way they may operate, were much less efficient in this annular rim than in any of the former modifications of the disc.

Experiment 13.—A disc of zinc was vibrated in this experiment, and the horse-shoe magnet, when employed, was laid on its edge on the stage, so that both poles were presented to the same side of the disc.

Without the magnet . . . . 120 vibrations.
With the magnet . . . . 90 vibrations.

Two bar magnets were next employed, the disc first vibrating between their north poles, and afterwards between a north and a south pole.

Experiment 14.—Between two north poles . . 112 vibration.
Between a north and a south pole 80 vibrations.

The bar magnets were next placed with both north poles on one side of the disc, and afterwards with both south poles on one side of it.

Experiment 15.—With north poles . . . . 109 vibrations
With south poles . . . . 102 vibrations.

I need not here remark that no other experiments in this interesting inquiry have presented such an extraordinary discrepancy of results as those I have just described. They prove in the most decisive manner that the energies of the magnet become curiously modified by every change of its position, with reference to the vibrating plate on which they are exercised; notwithstanding which, there does not appear to be any position in which it can be placed—provided it be sufficiently powerful—that would entirely neutralize its influence on the metal.

The most favourable position in which the magnet can be placed for displaying its influence appears to be that in which its north and south poles are presented to the opposite sides of the disc; and the position of the poles which appears to be the least favourable for such a display is when the poles that are presented to the opposite sides of it are of the same nature. This curious circumstance is very different from anything which I had observed in my experiments on discs of iron; for, with that metal, it had always appeared that when poles of the *same name* were presented to any point in the edge of the disc—the one above and the other below—the dipping needle invariably indicated the greatest polarity in the iron; and the least of all when the edge of the disc was placed between the poles of a horse-shoe magnet. Besides, the iron exhibits vigorous polarity whilst at rest; but not a trace of polar action could be detected in copper or zinc, unless those metals were in motion. The only opportunity, then, of discovering the distribution of polarity in them was whilst they were in that condition, either vibrating or revolving on an axis.

I began this tedious inquiry by suspending a magnetic needle near to a vibrating disc of copper, sometimes when the magnet was in one position, and sometimes when in another. The needle was evidently affected in whatever position the magnet was placed; but I soon found that it would oscillate with the plate whether the magnet was present or not. I might, perhaps, have expected that this would be the case, by taking into consideration that the needle itself would magnetize the disc. I observed, however, that the needle was not only more powerfully affected during the presence of the magnet, but that it would, in some positions, move in an opposite direction to that of the disc. Moreover, the deflections evidently varied by placing the needle on opposite sides, indicating something like a *north* polarity on one side of the disc and a *south* polarity on the other side. Neither were these appearances uniform in all parts of the same side; for in some places there appeared a north polar action, and in others as decided a south polar action. It would be unnecessary, however, to describe the various observations which I made by these first experiments: it will be sufficient to remark that it was soon discovered that this mode of experimenting was by no means the best adapted for such an inquiry; for although this arrangement of the apparatus showed most decidedly that the magnetic force in the copper was distributed in a very peculiar manner, yet, for want of command over the vibratory motions of the disc and

the needle, it was impossible to trace it with precision in the area of the metallic surface.

I next placed a disc of copper horizontally, so that it could be oscillated or rotated in its own plane on a vertical axis; and, by erecting a thin stage over it, a common compass needle could be placed over any part of its surface; and, as the axis was connected with a multiplying wheel and band, a motion of any required velocity could be given to the disc. By this apparatus most of my experiments were made; and I very soon found that no very great delicacy in the suspension of the needle was necessary, and that one mounted on a pivot was much better adapted for the investigation than one suspended by a silken film. The investigation, however, was exceedingly tedious, and required the most rigid observation to reconcile the phenomena to any determined law, or to trace the various curves on the surface of the discs, over which the needle would deviate in any required direction, with reference to these lines, by a constant standard direction in the motion of the disc; and what still further increased the difficulty, it was found that the distribution of the magnetic polarity varies with almost every difference in the dimensions of the plate. It also varies by the velocity, and again by the distance of the magnetic poles from the centre of motion; so that, upon the whole, although some rule may be observed by any one arrangement, yet the same rule is not applicable in all cases. A few experiments will show in what manner the distribution of polarity in the surface of the disc may be ascertained.

Experiment 16.—Let a copper disc, of about eighteen inches in diameter, be so placed as to be rotated in its own plane on a vertical axis; and let a horse-shoe magnet be placed with its south pole above and its north pole below the edge of the disc, reaching about two inches beyond the edge towards the centre. Let a compass needle be placed on a stage directly over the centre of the disc; and, by a proper arrangement of bar magnets, cause its south pole to be directed to the south pole of the horse-shoe magnet: turn the wheel, and the needle will move in the direction of the plate, but will not perform a revolution. If the vibrations of the needle be attended with corresponding motions of the plate, it may be made to sweep half a circle.

Experiment 17.—Let now the north pole of the needle be turned towards the south pole of the magnet, and again turn the wheel: in this case the needle will move in the opposite direction to that of the disc.

Experiment 18.—Let the needle be placed over, and just within the left edge of the disc, and not more than 90° from the magnet, Fig. 11. Let it also be permitted to repose with its axis at right angles to the diameter over which its pivot is placed. Turn the disc in the direction indicated by the large exterior arrow, and the south pole will be deflected towards the edge of the disc: reverse the revolving motion of the plate, and the south pole will be deflected towards the centre of it.

Experiment 19.—Let the needle be placed below the plate, and in the same vertical plane as before: the deflections answering to the motions of the disc in this case will be opposite to those when the needle was above.

In this way the needle may be placed opposite to various parts of the disc, and it will be found that the deflections vary in different places; and over some places no deflection will be observed by the motion of the disc in one direction, although a considerable deflection will be given by the motion being reversed.

It would be very difficult to account for these extraordinary phenomena by any known laws of Magnetics, and almost as difficult to reconcile them, with our present knowledge, to the laws of Electro-Magnetism. When these experiments were first intended to be published, I had arranged them under the head "*Polar Magnetic Streams*;" but I have since thought that the "*Distribution of Magnetic Polarity*" will be a much more appropriate term.

It would, however, be no great stretch of the imagination to suppose a disturbance of the electric fluid by magnetic action, as it would be only a kind of reaction to that which takes place in Electro-Magnetism. If this be really the case (although I do not at present assert that it is so), the electric force would rush from the magnetic poles in the direction of the small arrows in Fig. 11, when the plate rotates in the direction indicated by the large exterior arrow. This force is the most energetic on that side which is nearest the poles: it becomes diffused in the other parts of the disc, especially if it be large and attenuated so as scarcely to have any action on the needle on the extreme parts opposite to the magnet. It returns to the magnet again by various windings, and becomes more compact in proportion to its approach.

This is what the needle indicates to be taking place in the general surface of the disc, so that the deffections near to the left edge are different to those which are observed near to the centre on that side of the magnet. The force, therefore, appears as if it were first projected, or driven *from* the magnetic poles in an opposite direction to that in which the plate revolves, but soon divides itself into two distinct tides, which sweep the area of the plate, recurving to the poles again in opposite directions, as indicated by the two systems of arrows in Fig. 11.

The line of *greatest energy* is the resultant of the two systems of forces emanating from the left side of the magnetic pole, and is a curve determined amongst the feathers of the ascending arrows. It branches off with the aggregate of each of the two recurving systems of force, returning near to the edge on the left hand, but more in the area of the plate on the right hand side of the magnet.

There are also *neutral lines*, or lines in which the needle would constantly be arranged by the operation of these forces, if unsolicited by any other. These lines are determined at right angles to the resultant of the curve forces, indicated by the

arrows over which the needle is placed: their positions will, consequently, *appear* to vary with almost every variation in the length of the needle employed.

This curious distribution of magnetic polarity, or whatever force it may be that actuates the needle by the present arrangement, is decidedly peculiar to the direction of motion indicated by the exterior arrow, Fig. 11—there being no similar distribution, as in Fig. 12, by simply reversing the direction of revolution. If, however, the magnetic poles and the direction of motion be both reversed at the same time, then there is, on the upper surface, a distribution of force in every respect similar to that represented by Fig. 11. Hence, if in Fig. 12 the *north pole* of the magnet were to be placed *above* the disc, instead of the *south pole*, as there represented, the distribution of force would be indicated by the two systems of arrows in that figure—the revolution of the disc being in the direction of the large exterior arrow.

Now, as every condition, both of arrangement and motion, has been considered to be inverted to produce the distribution of force represented by Fig. 12, that figure may very well represent the lower side of the plate turned upwards, when the conditions of arrangement and motion are represented by Fig. 11. Indeed it is more convenient to examine the two sides of the disc in this manner, for when the needle is placed below, its motions cannot very well be observed, except at a short distance within the edge. When the plate is not very large, this force is more equally distributed over the surface, but in no case is it exactly so.

I have examined the distribution of magnetic polarity in discs and other forms of metallic surfaces with a good deal of attention, whilst the magnetic poles were variously posited with regard to them, and I have collected a number of curious facts, many of which are exceedingly difficult to arrange, on account of the singular windings of the force which actuates the needle. I have, however, succeeded in tracing the distribution, in some instances, by experiments which will be described in an early communication.

W. S.

Artillery Place, Woolwich,
March, 1832.

ON THE DISTRIBUTION OF MAGNETIC POLARITY IN METALLIC BODIES.

TENTH MEMOIR.

In the preceding Memoir I described the instrument (Fig. 9, Plate IX.) by means of which the experiments were first made, which indicated an extraordinary and novel distribution of magnetic polarity on the surfaces of copper and other non-ferruginous metallic discs. I also pointed out, though briefly, the method by which I detected the curiously winding force which actuates the needle on those surfaces when rotated between the poles of a horse-shoe magnet. In that Memoir, however, I described the distribution of the force no further than as it is developed by one condition of motion given to the disc—*i. e.* whilst it rotates in the direction of the large exterior arrow, Fig. 11, Plate IX. In continuation, therefore, I now proceed to show in what manner that force—still supposing it to be the electric—becomes distributed over the surface of the copper disc, when the rotation is carried on in the reverse order—the magnet still remaining in the same position as in Fig. 11, Plate IX. It may be necessary, however, in the first place, to make some further observations as to the manner by which I have been enabled to trace the curious windings which this force appears to take whilst in operation on the magnetic needle, or the mode by which I obtained the data necessary to the formation of the conclusions at which I have arrived concerning it.

In experiments 16 and 17, it is shown that when the needle is placed over the centre of the disc, and its axis in the same vertical plane as that joining the poles of the exciting magnet, it is a matter of no consequence in which of the directions the poles of the needle be placed: the deflections will depend upon the direction in which the disc is caused to rotate. For although the needle will in some cases follow the direction of the rotating disc, and in others travel the contrary way, according to the character of the pole which is directed towards the pole of the exciting magnet, still it will have a dependence upon the direction of motion given to the plate; so that if the position of the needle be such that its deflection will correspond with the motion of the plate when rotating to the right, its deflection will again correspond with the motion of the disc when the latter is caused to rotate to the left; consequently the deflections in the first case will be contrary to the deflections in the latter case. The same law will be observed when the position of the needle is such that the deflections are opposed to the direction of motion given to the disc; for if the needle travel

towards the left whilst the disc revolves towards the right, it will travel towards the right when the revolution of the plate is towards the left—manifesting in all cases that when the exciting magnet is stationary, the direction of the force which impels the needle entirely depends upon the direction of motion given to the disc.

This law being understood, we have next to contemplate the direction in which any selected pole of the needle travels whilst the disc is in motion; and a little reflection will make it readily appear, that in whichever of the two positions (Experiments 16 and 17) the needle may be placed whilst the plate is at rest, it will exhibit a *constant tendency* to assume some *determined new position* when the motion given to the plate is in *one certain direction;* and as constant a tendency to take up *some other* determined new position when the rotation of the plate is reversed.

To simplify this point still more, we will first suppose the needle to be placed as in Fig. 1, Plate X. *s n* being respectively the south and north poles. The needle in this position will travel in the same direction as the disc revolves (Experiment 16); and when the revolution of the disc is in the direction indicated by the large exterior arrow, the *south pole* of the needle will be deflected towards the *right* of the exciting magnet. Again, let the needle be placed as in Fig. 2, *s n* as before being the south and north poles respectively. In this case the needle will travel in the opposite direction to that of the revolving disc (Experiment 17): but in this, as in the former case—as will be observed by comparing the two figures—when the disc revolves in *one* and *the same* direction, as indicated by the large exterior arrows, the *south pole* of the needle has a constant tendency towards the right hand, as is shown by the small arrows pointing out the direction of its course. Hence the *new position* for the south pole of the needle, determined by the forces excited in the disc, by its revolving in this particular direction, is evidently on the right side of the exciting magnet, or to the right of an observer with the apparatus placed before him, as in Figs. 1 and 2.

This point being ascertained, the needle is now to be arranged, first a few degrees from the one and then a few degrees from the other of its positions, still keeping the *south pole* towards the right. The disc is to be put in motion (in that direction only indicated by the large exterior arrows, Figs. 1 and 2) whilst the needle is in each of the positions last given to it; and if the south pole now travels in the same direction as it did from both its former positions, it is plain that the excited forces still urge it towards some point, the situation of which is between those in which it was last placed. The needle is, therefore, again to be drawn still nearer to its destination indicated by the last trials, and the disc again put in motion in the same direction as before; the deflections are again to be observed, and the line, to which they indicate a tendency, to be still nearer approached by the position of the needle for the next trials. In this manner the line to which the excited forces of the disc urge the needle is to be gradually approached, and its true position at length correctly ascertained.

The deflections will gradually diminish, becoming smaller and smaller in proportion to the advances of the needle towards this *neutral line* ; and, when it is placed directly into the position of this line, the deflections will cease to be exhibited by the direction of rotation selected for this illustration—for the needle will now have a position of stability, or a position which the forces excited in the disc alone tend to preserve it in. If it vary only two or three degrees out of this line on either side, the slightest motion of the disc will urge it towards that line again; and, if the needle be made completely indifferent to the influence of any other forces than those excited in the disc, a deviation even of one degree on either side of the neutral line may be detected by a tendency which will be indicated to resume the position of that line again, whenever the plate is rotated in the proper direction.*

The process of experimenting is exceedingly tedious, but it is the only method by which the true position to which the forces excited in the disc tend to urge the needle can possibly be ascertained. And if those forces be electric, and endued with the same magnetic polarity as that exhibited by the forces of a Galvanic conducting wire, then the directions of the electric tides on the surface of the disc will be at right angles to the several positions which the needle is thus found to assume whilst the disc is in rotatory motion; and it was from numerous experiments and observations of this kind, whilst the needle was placed over various parts of the surface, that the necessary data were discovered, and the recurving forces carefully traced out.

The process by which the distribution of polarity on the surface of the disc has been determined being now understood, no further explanation will be necessary to illustrate the singular recurving directions of the excited forces which are supposed to actuate the needle on the upper surface of the disc, under the two conditions of rotation, than merely to refer to Figs. 3† and 4. The exterior arrows indicate the directions of motion given to the disc; and the two systems of small recurving arrows in each figure show the distribution and direction of the forces which impel the needle, and urge it to a position at right angles to the aggregate of any portion of those forces over which it may be placed during the revolving motion of the disc.

It will be observed, by comparing Figs. 3 and 4, that the direction in which the aggregate of the forces recurves is nearly, if not completely, reversed by simply changing the revolving motion of the disc. The arrows which indicate the direction of those forces are seen to issue from the front of the exciting magnetic pole in Fig. 3,

* I have been particular during this description in adhering to the effects of those forces which become excited by the disc revolving in one direction only, because it so happens that the two neutral lines indicated by the needle, whilst placed over the centre of the disc, are not coincident, but intersect each other at some considerable angle. Hence, although a position may be given to the needle from which it will not deviate whilst the plate revolves in one direction, a considerable deflection may be given by reversing the rotatory motion.

† Since my former communication went to press, I have had an opportunity of repeating my experiments on the surface of the disc, from the results of which I have been induced to offer Fig. 3, Plate X. as a more faithful representation of the distribution of the force in the central parts than that which is shown by Fig. 11, Plate IX.

but are re-entering at that point in Fig. 4. In the former figure also, the arrows are seen re-entering on both sides of the magnet, near to the edge of the disc; but in the latter figure the arrows issue forth from both sides of the magnet, along the same edge; so that the force in the edge of the disc is as decidedly reversed as it is in any part of the area, by simply reversing the revolving motion. This curious change in the direction of the force in the edge of the disc is beautifully illustrated by the following experiment.

Experiment 20.—Let the axis of the disc be placed horizontally east and west, and consequently the plane of it will be coincident with the plane of the meridian. Let the horse-shoe magnet be so arranged as to embrace the south edge of the disc between its poles, its plane horizontal, and coincident with that in which the axis of rotation is situated. Let also the north pole be opposite the east surface, and the south pole opposite the west surface of the disc, as in Fig. 5, N S representing the upper edge of the disc.

Let a magnetic needle be also arranged north and south, close beneath the lower edge of the plate. Rotate the plate in the direction which will carry its upper edge from south to north (from S to N, Fig. 5). In this case the disc will enter between the poles of the exciting magnet, under precisely the same circumstances, as in Fig. 11, Plate IX. and Fig. 3. Plate X.; the left edge in either of those figures corresponding with the *lower edge* in Fig. 5. The principal force which now operates on the needle will be that in the lower edge of the disc; and the direction of that force will be from north to south, or in the same direction as that in which the lower edge is in motion. (See Fig. 11, Plate IX. or Fig. 3, Plate X.) The south pole of the needle is deflected towards the *east* in precisely the same manner as it would be urged by the polarizing force of an electric current running from north to south through a conducting wire placed above the needle. The needle, N S, Fig. 5, shows the position into which it is carried whilst the disc is revolving over it.

Experiment 21.—Let the needle be now placed above the upper edge of the disc, and its axis in the same vertical plane, the rotation being continued in the same direction as before. In this case the force which operates on the needle is transmitted from north to south, the upper edge of the disc corresponding to the *right* edge in Fig 11, Plate IX. or Fig. 3, Plate X. The direction of the force is, therefore, the same in this experiment as in the last; but the needle is now placed *above* the edge, and the south pole is deflected towards the west.

If the disc be rotated in the contrary direction to that in which it proceeded in the two last described experiments, the distribution of the force will be represented by Fig. 4, in which case its direction in the edge, both above and below the magnet, Fig. 5, will be from south to north. The south pole of the needle, when beneath the *lower* edge, will be deflected towards the west; but when placed above

the *upper* edge of the disc, the same pole will be deflected towards the east; showing in a very beautiful and striking manner that the forces in the edge of the disc become completely reversed by simply reversing the revolving motion, and that the distribution of polarity is highly imitative of that which is displayed by the edges of a flag or cake of zinc, when partially heated at one end only;* the discovery of which, as I have before stated, gave me the first hint which led to the success at which I arrived in the investigation I am now describing.†

If two or three discs of the same diameter be placed close together on the same axis, so as to form a compound disc or plate, the forces which operate on the needle are much more powerful than when one disc only is employed. Much, however, depends upon the thickness of the metal—thick discs having a great advantage over those which are thin, notwithstanding which a decided uniformity in the distribution of polarity is displayed even in the thinnest copper or zinc foil.

I made a compound disc by soldering the edges of two single ones to a rim or hollow cylinder of copper, about half an inch deep, so that, when completed, it formed a cylindrical box, half an inch high, and about ten inches in diameter, having a perforation through its centre for the introduction of the spindle on which it was intended to rotate. When this cylinder was mounted in the place of the single disc, in Experiments 20 and 21, the deflection of a four-inch needle (neutralized in the usual way) would amount to about 40° with a moderate velocity of rotation. When the velocity was considerable, and the motion equable, the needle would be perfectly steady at that, or even a greater angle of deflection.

Straight needles, particularly when they are very long, are by no means well adapted for obtaining the greatest effect from the forces in the edge of the disc, whilst rotating in a vertical plane, because of the great distance at which the poles are necessarily placed from those operating forces. It is much better to employ needles which are bent into circular arcs, having nearly the same curvature as the edge of the disc. Two needles of this form may be advantageously employed at the same time, the one above and the other below, and both concentric with the edge, as in Fig 6. The needles are attached to a straw, or thin slip of light wood, with their poles placed in opposite directions. When thus arranged, their directive force will, in a great measure, be neutralized, both as regards the Magnetism of the earth and that of the exciting horse-shoe; and as the actuating forces in the edge of the disc operate in the

* See second Memoir, page 89.

† At the time I was making these experiments, I found that the frame of an electrical machine with a multiplying wheel and band was very convenient for giving the disc a considerable velocity in a vertical plane. A spindle, supported in the pivot-holes of the frame, and furnished with a pulley at one end, carried the revolving disc, and a pile of books formed the stage for the support of the horse-shoe magnet. Some time last summer, however, I constructed an apparatus for the purpose of rotating discs, cylinders, &c. on a horizontal axis, which, as it very much resembles a *plate machine*, it is not necessary to describe in this place, any further than merely to mention that it is furnished with neat stages for the support of the exciting magnet and the compass needles.

same direction, both needles will be impelled in one and the same way; so that whatever may be their position when deflected, they will constantly appear in the same vertical plane. The arrows in Fig. 6 show the direction of the aggregate forces in the edge of the disc, when it is rotated in the direction as shown in Fig. 3.

The singular and complicated distribution of the force discovered in these rotating discs of copper, led me to undertake some other experiments, by means of which I considered it possible that I might arrive at some simple law which would disclose the novel and apparently mysterious arrangement; for, whether the phenomena emanate from magnetic or from electro-magnetic action, there appeared to me to be no law yet discovered in either of these branches of research, that would produce a distribution of polarity like that which I have pourtrayed in Figs. 3 and 4; notwithstanding which, the uniformity of the distribution which became manifest at every repetition of the experiments left no doubt as to the immutability of some law, to the operation of which the regularity of the distribution was entirely owing.

In this investigation it was necessary to take into consideration the various directions which different parts of the revolving disc assume with regard to the exciting magnet; for, as the poles are not placed in the centre of motion, it is plain that whilst some parts are advancing towards them other parts are receding from their vicinity: some parts, again, are crossing the magnet to the right, whilst others are crossing it towards the left. All these motions in the disc are going on at the same time; so that, upon the whole, the apparent complexity of the problem put any inquiry concerning it rather in the position of a "forlorn hope" than of anything like certainty of success.

Considering, however, that as the vicinal regions of the disc must necessarily receive the exciting impressions in a much higher degree than those more remotely situated from the magnetic poles, it might be expected that if any satisfactory conclusions were to be arrived at, those parts of the disc the most powerfully excited were more likely than any other to afford the necessary data. My inquiries were, therefore, more particularly directed to the investigation of that *half* of the disc nearest the magnet, the curvilinear direction of which, with regard to the exciting pole, is easily resolved into four rectilinear motions.

Let *m o*, Fig. 7, be the constant radius situated between the magnetic poles; then the diameter, *n n'*, drawn at right angles to the former line, will be the line of demarcation which separates the disc into the two required halves.

Now, when the disc revolves in the direction of the exterior arrow, the quadrantal portion, *m o n*, will advance towards the pole, *m*, whilst the quadrantal portion, *m o n'*, will recede from it.

Let *c o* be any radius of the disc approaching the magnet, *m*; then, in order that any point, *c*, in that line may arrive at *m*, it must necessarily partake of the direction,

$c\ b$, which would bring it towards the *side* of the magnet; and also of the direction, $b\ m$, which would carry it to within the magnetic poles: and as the lines, $c\ b$ and $b\ m$, are respectively the exact measure of the spaces through which the point, c, would have to travel in those directions, whilst approaching the magnet, and are both performed in the same time. They are also the faithful representatives of the respective *mean* velocities with which the point, c, is carried in each direction whilst advancing from c to m.

Now, as $c\ b$ and $b\ m$ are respectively the sine and versed sine of the angle, $c\ o\ m$, the mean velocity from c to m, in each direction, will always be proportional to those lines, from whatever point of the quadrant the point, c, has to travel. If c travels through an arc of 90^o, or from n to m, the mean velocity in each direction will be equal, because $n\ o = o\ m$; but if the arc be less than 90^o, the mean velocities will be unequal. If the arc $n\ c$ be 45^o, the mean velocity from n to c will be in favour of the direction $o\ m$; but between c and m the predominating velocity will be in the direction of $c\ b$.

Now as the excitation is more powerful in the neighbourhood of the magnetic poles than in any other part of the disc, the vicinal area, $c\ o\ m$ of the quadrant $n\ o\ m$ will constantly be receiving stronger impressions than the remote area, $n\ o\ c$. And as the predominating mean velocity in the area $c\ o\ m$ is in the direction $c\ b$, the ascendant influence will consequently be due to that direction of motion.

With regard to the quadrantal area $m\ o\ n'$, nothing more appeared necessary to be understood than to resolve its curvilinear motion into rectilinear directions in the manner already considered in the other part of the disc, supposing it to be receeding from the magnet instead of advancing towards it.

Under these considerations the experiments necessary for inquiry, which at first view had appeared to present considerable difficulty, became very much simplified, being reduced to four rectilinear motions of the plate—attending to the velocity in each direction, and taking into calculation the observed phenomena under each indidual circumstance.

The experiments were made with a rectangular plate of copper, about 18 inches long and 12 inches broad. This plate was placed between the poles of a horseshoe magnet, and moved in a horizontal plane. The upper surface of the plate was exposed to the action of the *south pole*, and consequently the lower surface to the action of the *north pole* of the magnet.

Nothing more will be necessary to describe the distribution of the force which operates on the needle, whilst the plate is in motion in the four selected directions, than merely to refer to Figs. 8, 9, 10, and 11. The exterior arrows in each figure indicate the direction in which the plate is moved, and the curved systems of arrows show the distribution of the force.

In Fig. 8, the distribution is similar to that shown in Fig. 11, Plate IX, or Fig. 3, Plate X.; and the motion of that part of the metal under the strongest excitation in both cases is in the same direction—*i. e.* from left to right. The same comparison may be made between Figs. 9 and 4, where both move from right to left between the poles of the exciting magnet.

In Fig 10 the plate is introduced directly into the interior between the two limbs of the magnet; and in Fig. 11 it is withdrawn in the same right line. The distribution of the forces by these two motions are simple curves, having only one direction in each. In each case, however, the curves have every appearance of being continuous, running into themselves between the poles of the magnet, and forming complete vortices round a central nucleus or narrow space joining the exciting poles.

Now as the distributions in Figs. 10 and 11 are simple vortices, they may be applied to explain the compound distributions in the other figures. Let it be supposed that each system of arrows in Figs. 10 and 11 represents a complete vortex of the force, and let an observer be supposed to be placed in its centre; then as the plate advances towards the poles, as in Fig. 10, the direction of the force in every point of the vortex will be towards the left hand; but when the plate recedes from the magnetic poles, as in Fig. 11, the direction of the force will be towards the right hand. These are simple elementary vortices.

Apply now each of these elementary vortices to Figs. 8 and 9: in each figure the plate is both advancing and retiring from the pole at the same time. In Fig. 8 the plate is advancing on the left side of the magnet, and the vortex flows towards the left hand of an observer placed in the centre of its motion. On the right side of the magnet the plate is retiring from the poles, and the vortex is flowing towards the right hand, or in the same direction as in the elementary vortex in Fig. 11. In this way the elementary vortices in Figs. 10 and 11 will explain the compound distributions of force in each individual case, as represented in the figures.

In Fig. 3 and 4, where the disc revolves on a centre, the excitation arising from the motion being in the direction, *o m*, on one side of the magnet, Fig. 7, is counteracted by the opposite excitation on the other side of the line, *o m*; for, as on one side of the magnet the motion would be advancing, and on the other side retiring, as in Figs. 10 and 11 respectively, the forces arising therefrom would nearly, perhaps completely, destroy each other. It is possible, however—nay, it is even probable, that all the systems of forces arising from the four rectilinear motions are in play when the disc is revolving on its axis; but the insignificancy of the two last contemplated forces, with regard to those which are due to the motions indicated by Figs. 8 and 9, must necessarily render them exceedingly inefficient. If the force be electric, it is likely that the remote parts of the disc serve merely as conductors to that excited in the parts vicinal to the magnet.

The small curved arrows in Figs. 12 and 13 indicate the distribution of the force in annular discs of copper or zinc, when rotated on an axis in the manner described for complete discs. The large exterior arrows indicate the direction of motion in each figure. The distribution in these annular discs is precisely the same, so far as the metal permits, as that in complete discs.

Fig. 14 is intended to show the position of the neutral line on the rectangular plate, when moved in the direction of the arrow, between the magnetic poles. The arrow is a right line, crossing the magnetic pole and two inches in front of it. The small needles are placed an inch from each other, and their positions, with regard to the arrow, show the inclination at each station, or the position in which the excited forces in the plate alone would place them.

W. S.

Woolwich, July, 1832.

ON THE THEORY OF MAGNETIC ELECTRICITY.

ELEVENTH MEMOIR.

The original plan which I had prescribed to myself for the publication of my investigations on the distribution of magnetic polarity in metallic bodies, was that of first describing all those experiments which appeared to me to be the most interesting, with such explanatory remarks and practical rules for their exhibition, as were necessary to their being properly and easily understood; and afterwards to offer such theoretical inferences, with observations, as naturally presented themselves to my mind whilst contemplating the curious and novel phenomena which these inquiries elicited: and in order that the arrangement might be the more regular, uniform, and intelligible, I placed the experiments on iron in the earliest part of the detail. According to that plan there would have been another communication previous to that which I am now writing, which would have continued and perhaps completed the detail of my former original experiments. Since sending my last communication to the press,* however, I have had an opportunity of perusing a paper containing the detail of more recent experiments of Mr. Faraday, published in the *Philosophical Transactions* for the present year; and finding that several of the experiments there detailed, although performed with somewhat different arrangements of apparatus, are intimately connected with those of mine already published, and consequently with those which I have not yet described, I have been induced to deviate from my

* Philosophical Magazine and Journal of Science, vol. i. page 31.

original plan, and to offer more early in the series than I intended those *theoretical elements* of this new branch of physics, of which all the *rules* hitherto advanced for the exhibition of the phenomena, however important they may appear in a practical point of view, are but the mere consequent subordinate results.

Before proceeding further, however, with the principal object of this communication, I must beg permission to observe, that notwithstanding the *title* under which I have hitherto published my investigations on this subject is perfectly unobjectionable, and also sufficiently comprehensive and explanatory for all the phenomena exhibited by the deflections of the magnetic needle, the more recent discoveries of the electric spark, and other electrical phenomena by the same mode of excitation (which have completely verified my anticipations as to the real character of the excited force which operates on the needle), require to be arranged under another and a very different head. *Magnetic Electricity* is an appellation which comprehends, and may very conveniently serve to express *generally*, every class of phenomena hitherto developed by magnetic excitation of the electric matter, whatever may be the character or form of the metal employed. It will, therefore, be more consistent with simplicity to confer on the whole that general appellation, and to designate, if necessary, each individual class of phenomena by its respective characteristic properties. Precedents of this kind, distinguishing various classes of phenomena, are abundant in scientific nomenclature, and cannot in this instance be reasonably objected to.

Considering, therefore, that *Magnetic Electricity* is an appellation at once emphatic, intelligible, and expressive of the exciting agent, I have been induced to publish my *theoretical* views of this subject under that general head. Moreover, it so happens that the laws of this species of electric excitation are not peculiar to the display of one class of phenomena only, but are applicable to the development of every fact hitherto discovered in this branch of physics. It does not, therefore, require that one mode of excitation should be observed for the production of the electric, and another mode for the production of the magnetic effects, but merely a diversity in the arrangements of the apparatus; for whatever be the character of the phenomena to be exhibited, the same laws of excitation are uniformly to be observed—a circumstance which affords another and very powerful argument in favour of the adoption of the general significant appellation "Magnetic Electricity."

Researches in Magnetic Electricity have hitherto been confined to the disturbing of the natural equilibrium of the electric fluid residing in metallic bodies, and perhaps other conductors of Electricity, by means of certain movements of those bodies, with regard to natural or artificial magnets; and some very curious facts have been discovered by these modes of experimenting.

It is certainly something to discover new facts, and something more to point out rules by means of which the novel phenomena may be uniformly exhibited. It very

often happens, however, that in this stage of inquiry the development of the most beautiful and interesting part of the science is but half accomplished. There is still something more to be done: a process of ratiocination has yet to be exercised, frequently above the sphere of the mere experimenter, which conveys the ideas far beyond the simple exhibition of phenomena. Such sublime investigations, if successful, unfold, and penetrate into, the more recondite recesses of nature—transport the mind to the very source from which emanate proximate and unnerring fundamental laws, and display in superior radiance of philosophic light the *modus operandi* by which the dormant powers are impelled into activity, and exercise their dominion over the resulting obsequious phenomena.

I believe it is generally admitted by writers on Magnetism that a steel bar in a state of polarization is surrounded on every side by the magnetic matter, frequently called the "*Magnetic effluvium*," which forms to the bar a species of magnetic atmosphere. This point being granted, it will be a matter of no consequence to the present undertaking whether this effluvial matter be stationary as regards the magnet, or whether, as some have imagined, it be continually flowing from pole to pole: it will be sufficient for the present purpose to consider it as consisting of exceedingly minute, polarized particles, emanating immediately from the surface of the steel*—concessions of no novel character, and such, I imagine, as but few will be found willing to deny.

With regard to the *distribution* of the virtual intangible magnetic particles in the vicinity of the bar, we cannot, perhaps, be more correctly directed for information than by examining attentively the arrangement of fine particles of iron, when gently and promiscuously scattered on paper, beneath which is placed a magnetic bar; for, notwithstanding the magnetic matter itself—in consequence, perhaps, of the exceeding minuteness of its particles—escapes the cognizance of vision, the distribution of the ferruginous particles being accomplished by its polarizing efficacy, may very justly be considered as the true representative of the distribution of the virtual intangible magnetic matter enveloping the surface of the steel.

Now, as those elemental magnetic intangibles are polar, their poles will necessarily be arranged according to the immutable laws exhibited by visible tangible magnets, to which they are the main-spring of all their energies, and the only active agents by which their mysterious phenomena are called forth, as displayed in the silent motions of the passive obedient steel. Regular concatenations of alternate *north* and *south* poles will, by their mutual attraction, pervade every part of the magnetic effluvium as decidedly and as uniformly as in a consecutive series of polarized ferruginous bars.

Under these considerations it will readily appear that all the elemental magnetic particles enveloping the *north portion* of a regularly magnetized bar of steel will have

* This was the expression I used when this theory first appeared in the *Philosophical Magazine*. I have since then, however, considered that the Magnetism of the steel polarizes the magnetic effluvium residing in the vicinal space on every side.

their *south poles* directed towards the surface of the metal, and consequently all their *north poles* will be directed outwards in every part of the arrangement. Precisely the reverse of this distribution of poles will take place in the magnetic matter enveloping the *south portion* of the steel; so that, in this case, the *north poles* will be directed towards the south portion of the metal, and consequently all the *south poles* will be turned outwards.

If now we contemplate the arrangement which would take place in the vicinity of *one* polar portion only of a piece of steel, supposing it to be uninfluenced by a pole of the other kind, we shall discover, by the laws of magnetics, that the polar affections of the enveloping magnetic matter will arrange the particles of which it is composed into radial *polar lines*, emanating from every part of the steel surface; for, as each individual line will be formed by the attachment of a consecutive series of dissimilar poles of elementary particles, the remote extremities of all these virtual *magnetic lines* will become similarly polarized, in consequence of which they will have a constant tendency to diverge from each other. Hence, if we be contemplating the *north polar* portion of the steel, the remote extremities of the virtual *magnetic lines* will be *north polar;* but if it be the *south* portion of the steel which comes under consideration, the remote extremities of the *magnetic lines* will be *south polar.* Hence also the lines of magnetic action, which envelop a bar of steel, displaying two poles only, may be divided into two distinct classes or systems—one of which may be called north polar and the other south polar. If it were possible that either of these systems of magnetic lines could be displayed separately and independently of the disturbing force of the other systems, those lines would be perfectly straight, or without flexure in every part of their course—that is, they are naturally *right lines;* and if the magnetized body were a sphere, the virtual polar magnetic lines would radiate in right lines from every part of its surface. (See Figs. 1 and 2, Plate XI.)

Hitherto I have endeavoured to explain what I consider radiating *magnetic polar lines*, emanating without obstruction from a magnetized piece of steel, under the supposition of its being unipolar on every part of its surface; but as no piece of steel, of whatever form it may be made, has yet been known to exhibit one uniform polar state, but, on the contrary, each piece of magnetized steel invariably displays a plurality of poles, and one at least of each description—it will next be necessary to take into consideration in what manner the two systems of polar matter affect each other, and in what manner the elementary polar lines of each system become deflected out of their natural rectilinear course by their mutual attraction of each other.

If fine steel or iron filings be gently scattered on a sheet of card paper, under which is placed a bar magnet, they will immediately become polarized by the influence of the magnetic matter enveloping the bar; and if they be slightly agitated by hitting the paper a few gentle taps with a pencil or other such light body, they will become

arranged in multitudes of exceedingly fine lines, some of which will be straight and others curved, as in Fig. 3. Conspicuous lines, each with a dash across one of its extremities, are drawn to show their general positions in each system.

In this arrangement of the ferruginous particles we have, perhaps, a pretty correct picture of a longitudinal section of the distribution and arrangement of the intangible magnetic matter enveloping the steel bar. Near to, and around the extremities of the bar, the two systems of *polar lines* proceed nearly in their natural rectilinear direction; but those *polar lines* of each system which are more vicinal to the neutral point, or to the neutral line, *e q*, which crosses the centre of the bar, in consequence of presenting poles of different characters outwards, do, by their mutual attractions, aberrate from their natural course, and bend or incline towards each other; forming curves of different degrees of flexure, according to the powers of their reciprocal forces, and their distances from each other. If the steel bar be cylindrical, and uniformly magnetized on every side, then, whatever longitudinal line of this magnet be turned upwards, or towards the paper, a similar arrangement of *polar lines* will be exhibited, demonstrating in the most satisfactory manner that the virtual polarizing magnetic matter completely envelops the ferruginous cylinder. Figs. 1 and 2 represent the distribution of fine particles of iron when strewed on paper above the ends of the cylindrical magnet.

Fig. 4 is a representation of the arrangement of fine particles of iron, strewed on paper above a horse-shoe magnet, which affords a tolerably exact idea of the direction of the invisible *polar magnetic lines* as they are distributed in the plane of the magnet. Fig. 5 represents the arrangement and distribution of iron filings scattered over a transverse section, or over the poles of the same magnet.

In Fig. 4 it is observable that the *magnetic polar lines* exhibit the greatest degree of aberration from their natural rectilinear direction in front of the metallic poles, whilst but very trifling deflections of the polar lines are to be seen along the inner edges of the magnet; even on the outside of the limbs the aberrations are much less than those exhibited on the surface of the bar magnet, Fig. 3, at similar distances from the poles.

In this case the *magnetic polar lines* maintain their natural rectilinear direction, even at considerable distances from the extremities of the metal, and particularly between the limbs of the magnet, in consequence of the two systems emanating from the metallic surface in diametrically opposite directions, and meeting each other, as it were, in the same rectilinear path. On the outside of the limbs the aggregate of the two systems of polar lines, in the plane of the magnet, are not only so far separated from each other as to be little affected by their mutual attraction, but are also so situated with regard to the transverse curvilinear forces (see Fig. 5), that they form a series of resultant lines of trifling flexure in the plane of the magnet. These lines have,

however, a small degree of flexure from their natural course, arising from their mutual attractions in the direction of the metal, which bend them a little towards the centre of the magnet.

Having thus illustrated what I consider to be the virtual *polar magnetic lines*, and also their most usual arrangements in the vicinity of steel, or other ferruginous magnets, I now propose to show that the *excitation* of Magnetic Electricity, and also the *direction* of the currents excited, are referrible to the *agency* and *position* of these *polar magnetic lines* alone, without any regard whatever to the poles, figure, or position of the steel which they envelop, any further than as those lines are casually arranged on its surface by the diverse arbitrary forms and proportions it is so frequently made to assume.

The theory of electric excitation by magnetic agency will be embraced in the following positions:—

Position 1.—Magnetic Electricity may be excited in all the metals, and perhaps in some other conductors of Electricity.

Position 2.—The excitation depends upon a disturbance of the equilibrium of the electric fluid natural to the metal, by its impinging on the exciting *polar magnetic lines;* and is accomplished by mechanical motion, either of the metallic body to be excited, or of the magnet, or of both at the same time. For simplification, however, we will suppose the magnet to be stationary, and the metallic body alone to be put into motion.

Remark.—As the electric fluid by this process has not as yet been recognised in any other state than that of motion, the phenomena are necessarily displayed upon the principles of *Electro-Dynamics*. Hence the term "*excitation*" in this place is to be considered not only expressive of a process for simply disturbing the electric fluid, but as one which is capable of communicating to various quantities of it an infinite variety of velocities. And as the *quantity* of fluid in motion, and the *velocity* with which it moves, will conjointly constitute an *electro-momentum*, which at all times will be proportional to the product of its constituent elements; it is, therefore, the production of the *electro-momentum* which is to be understood when we speak of various degrees of excitation.

Indeed, whatever may be the nature of the exciting agent, or the mode of its application, it is in this sense only that the term "*excitation*" can, with any degree of propriety, be applied when electric currents and their effects are the phenomena under contemplation. "Electro-momentum" is an expression which at once conveys to the mind the author's meaning—that it is the production of the *velocity* multiplied into the *quantity* of electric matter which, by the process, whatever may be its character, is impelled into motion from its previous statical repose.

Electric currents generated by a Voltaic battery are constituted of distinct alternate charges and discharges of the electric matter, or of *electro-pulsations*, and may be assimi-

lated to the currents of blood through the animal system, which are produced by the alternate charges and discharges at the heart; and it is very far from being improbable that both are actuated upon the same principle. The electric fluid called forth by a Voltaic battery is, therefore, alternately accumulating and discharging during the whole time the instrument is in action. In the former case the *intensity* is exalting; but it is in the latter alone that the force is exhibited—which force is the production of the *quantity* of fluid discharged, and the *velocity* with which it moves conjointly, which may very clearly be understood by the term "*electro-momentum*."

As, however, the *electro-pulsations* in most cases are produced too rapidly to be separately considered, it is the *aggregate* of the multitudinous *electro-pulsations* constituting the general discharge that is to be understood by the term "*electro-momentum*," when a Voltaic battery is the instrument employed for generating the electric currents.

Thermo-electric currents are also, in some cases, of a pulsatory character; for, as several of the metals are constituted of crystals, and those crystals of distinct elementary metallic films (see second Memoir), the heat, which in this case is the impelling agent, must necessarily arrive at a certain degree of *concentration*, or of *intensity*, if you please, in *one* film or distinct metallic element, before it can possibly take possession of the next: consequently, however small and inappreciable may be the interruption in each stage of its progress, each interruption must necessarily produce a virtual *pause*, the very existence of which in the advances of heat from film to film will constitute a pulsatory progression.

In the "Marechausian"* (*colonne pendule*) or dry electric column, the electro-pulsations are, in consequence of the very great number of interrupting papers, less frequent than in either the process of Volta or in that of Seebeck—notwithstanding which, the instrument produces slow pulsatory currents.

Position 3.—When the metallic body moves in any given direction with regard to the *polar magnetic lines*, the more *rapid* the motion the greater will be the degree of excitation or *electro-momentum* produced; and *vice versa*, the slower the motion the less will be the degree of excitation: consequently, when the velocity is at a minimum or nothing, no excitation whatever can exist.

Illustration.—If the excited body be of such dimensions as to have the whole of its natural electric fluid put into motion by the process, the *electro-momentum* would always be proportional to the *velocity*, because of the *quantity* of fluid in motion being constantly the same: and as by Position 2, the motion of the fluid depends upon the motion of the excited metal, the velocity of the former will at all times depend upon that of the latter; and consequently the *electro-momentum* or extent of excitation will be proportional to the velocity of the moving body operated on.

* M. Marechaux appears to have constructed the first dry electric column.—*Ann. de Chim.* for January, 1806.

There may possibly, however, be a limit to the extent of excitation by an increase of motion, when the velocity is very great, in consequence of the yielding of the excitating *magnetic lines* to the force of the moving body, or to its electric fluid whilst striking them with great rapidity. But as far as my experiments and observations have been conducted, I am led to believe that the *electro-momentum* may be exalted by an increase of motion until the velocity becomes exceedingly great.*

Position 4.—When the velocity of the moving body, and energy of the exciting *polar magnetic lines* are constant, the *maximum* of excitation will be accomplished by the body moving at *right angles* to those lines against which it impinges.

Position 5.—When the direction in which the body moves is inclined to the axis of a group of *polar magnetic lines* at any other angle than 90°, it receives no more excitation than what is due from the quantity of its motion taken in the direction perpendicular to that axis.

Illustration.—As the excitation of the body, or of the electric fluid which it contains, depends upon its collision with the polar magnetic lines, the greater the number of those lines against which the body strikes in a given time, the greater will be the number of exciting impressions accomplished in that time.

Let $a\ b$ and $a\ c$, Fig. 6, be two directions in which a piece of metal is caused to move, the former perpendicular and the latter oblique to the axis of the group of *polar magnetic lines*, represented by the vertical lines dashed across their heads in the figure. If, now, $a\ b = a\ c$ represent the velocity in each direction, then those two lines will also represent the spaces through which the body moves in two equal portions of time. Now it is evident, by mere inspection of the figure, that whilst the body moves from a to b, in the direction *perpendicular* to the axis or general direction of the *polar magnetic lines*, it will have to impinge against a greater number of those exciting lines than whilst moving in the oblique direction from a to c. Or the body will impinge on no greater a number of *polar magnetic lines* whilst passing obliquely from a to c, with the velocity $a\ c$, than it would strike by moving with the less velocity $a\ d = f\ e$, the quantity of its motion taken in the perpendicular direction, $a\ b$.

But, as the velocity is supposed to be constant in both directions, then the same number of exciting impressions will be accomplished by the body being in motion during a *part*, $a\ d$, only of the time, $a\ b$, in the perpendicular direction, $a\ b$, as will be accomplished by its being kept in motion the whole of the time $a\ c = a\ b$, in the oblique direction, $a\ c$.

Corollary.—Hence it is evident that if a metallic body were to move in the direction of the axis of a group of parallel *polar magnetic lines*, it would suffer no excitation whatever. The position is also conformable to experiment.

* Since this theory was first published, it has been discovered that great velocities deteriorate the excitement. (See fifteeth Memior.)

Position 6.—The natural or primitive *channel* of an electric current, generated by magnetic agency, is at *right angles* to the axis of the exciting *polar magnetic lines*, whatever may be the direction in which the exciting body moves.

Remarks.—The current may, however, be led or conducted in various other directions according to the figure and dimensions of the metal employed, and the various directions in which it may be put into motion; notwithstanding which, the *primitive* channel of the current will be constantly the same—at *right angles* to the axis of the exciting *polar magnetic lines.*

Position 7.—The *direction* in which the current *flows* with regard to the exciting *polar magnetic lines* is constantly the same, whatever may be the direction in which the metal is put into motion, or to whatever extremity or other part of a magnet the metal may be applied.

Illustration.—Let *a b c d*, Fig. 7, be a ring of metallic wire, placed with its plane horizontal, and embracing a bundle or group of *polar magnetic lines*, the axis of which passes through the centre of, and at right angles to, the plane of the ring. Let those *magnetic lines* emanate from the *south* magnetic pole of a bar of steel, placed beneath the paper on which the figure is drawn; consequently their *south* poles (marked poles) will be upwards, and may very conveniently be represented by the group of small crosses embraced by the ring. (Fig. 8 is an oblique view of Fig. 7.)

If now the ring be put into motion in its own plane—it will be a matter of no consequence which side advances towards the centre—the electric current, thus excited, will *flow* in every part of the ring in *one* and the *same* direction—which direction is indicated by the four exterior arrows, Fig. 7.

Now, as the group of *polar magnetic lines* is stationary and encompassed by the ring, it will be *that part* only of the ring, which *advances* towards the centre or axis of the group, which will receive the exciting impressions. The opposite side, instead of impinging on the *polar magnetic lines*, absolutely recedes from them, and operates in no other capacity than that of conductor to the excited current in the advancing side. So that whether it be *a b c* or *d* which advances towards the centre, their opposite sides, *c d a* or *b*, will respectively recede from the axis of the group, and become conducting parts of the ring, whilst the former co-relative parts are receiving the exciting impressions.

Fig. 9 represents the ring cut open in four places, and each part made perfectly straight, to represent four separate pieces of wire.

Let any one of these wires advance towards the centre of the group of *polar magnetic lines:* then, as the excitation in this case is under precisely the same circumstances as in the former, the electric current in the advancing wire, or part of the ring, is also constant and uniform in its primitive direction, flowing in one and the same invariable course, relatively to the exciting *polar magnetic lines* which gave it birth and activity. (See arrows in Fig. 9.)

To familiarize still further this beautiful law of Magnetic Electricity, let any man suppose himself to be placed in the axis of a group of *polar magnetic lines*, similarly situated to those in Figs. 7 and 8. Let him now stand, or suppose himself to be standing, in the centre of a hoop or ring of metal. Whilst in this position, let him permit the ring to move in its own plane; consequently some part of it will advance towards him, whilst the opposite part will recede from him. The former will receive the exciting impressions, and the latter will become a portion of the conducting circuit.

Let him now look to whatever side of the ring he pleases, the current *before* him will be flowing from his right to his left hand. If it be the excitation of a straight wire which he is contemplating, let him consider it as a portion of the original ring, or as one of the straight pieces in Fig. 9, permitting it to advance towards his front: his *left hand* will be the unerring index to point out the direction of the passing electric current.

A walking-stick, or any other such article, may very well represent the metal to be excited: then a person standing in the position of the polar magnetic lines, as represented in Figs. 7, 8, and 9, and holding the stick before him by its extremities, one in each hand, and at *right* angles to the axis of his person, or to a straight line drawn from his head to his feet, will, by pulling the stick towards him, show the proper direction of motion for effecting the greatest degree of excitation under the conditions laid down in Position 4; and, by the illustration of Position 7, the current would flow through the stick from the right to the left hand.

The preceding positions will, if I have not deceived myself, exhibit a correct view of one class, at least, of the natural elements of Magnetic Electricity—viz. those *secondary* theoretical laws which govern its excitation and give direction to its polar streams. They are those *proximate* laws by which the display of the phenomena is accomplished and regulated, and by which it may very simply be explained and easily understood. By these laws the experimenter may be directed in his manipulation, and with precision he may foretell the direction of the resulting electric streams.

It is probable, however, that other laws are in operation during this novel process of excitation which are still more remotely situated from observation, and require for their development experiments and a mode of reasoning of a very different order to those which have been employed for organizing the system of *proximate* laws already explained.

It appears to me that electric currents generated by magnetic agency are not the *immediate* effects of the magnet employed in the excitation. It is highly probable that there is a *mediate*, or intervening agent called forth—the Magnetism natural to the excited metal—which, by being polarized by the exciting *polar magnetic lines* of the magnet, becomes the *immediate* agent in giving life and energy to the previously dormant Electricity of the metal.

Remote and mysterious as the intermediate agency of the natural Magnetism of the metal, in this process of exciting Electricity, may appear in the present infantile stage of the science, I have much reason to suppose that such is the fact. The phenomena in Magnetic Electricity, as well as those in Electro-Magnetism, are highly favourable to the hypothesis; and I am not aware of an exception that militates directly against it. Moreover, the facility with which the *modus operandi* might be explained, upon the simple principles of polar magnetic lines alone, would, I am persuaded, establish a degree of plausibility, at least, not easily shaken by any counter-reasoning likely to be advanced; and the illustrations which it would be possible to bring forward in support of such a hypothesis might possibly be the means of fixing a basis on which the theory of excitation in this curious branch of physics is eventually and permanently to be established.

The same class of *remote* laws apply equally to Electro-Magnetism as to Magnetic Electricity; and it would be very difficult indeed, independently of those laws, to completely harmonize with each other the phenomena displayed by the two different modes of excitation.

With regard to Electro-Magnetic action, the idea can hardly be said to be novel. Mr. Buxton long ago asserted that the Magnetism of the conducting wire becomes polarized, and is the intermediate agent between the transmitted electric current and the magnet employed; but the illustrations which have been advanced by that gentleman might possibly require considerable modification to establish a theory on those principles. (*Scientific Gazette*, for September, 1825.)

I have heard brought forward, as an argument against the hypothesis of magnetic polarity of the conducting wire, an experiment of Sir Humphry Davy's, which showed the deflection of an electric current passing through air, between the charcoal points of a Voltaic battery, by the presentation of a magnetic pole. Such arguments can have but very little force in discussions of this character, for the experiment develops nothing different to the generality of electro-magnetic phenomena. If an electric current be capable of rousing into activity the dormant magnetic powers of ferruginous matter, no doubt can possibly be entertained of its susceptibility of being put into motion by the energies of an already formidable polarized bar.

This is the extent of reasoning to which the experiment can be applied, even under the supposition of the electric current being the immediate agent in the process of magnetizing iron or steel, and that no intervening polarization of the conducting wire is concerned in the operation; which, in fact, is no argument whatever, further than might be advanced from any other electro-magnetic experiment.

On the other hand, it might be inferred, with a great deal of propriety, that if the electric current is capable of calling forth the latent Magnetism of hard steel, in which it is pent up and retained with a degree of vigour which requires the greatest efforts

of the exciting agent to extricate it and accomplish its polarity even to a comparatively small extent, it is but reasonable to expect that in those metals which do not possess so exalted a degree of retension as hard steel the *same* exciting agent would accomplish a polarity to a much greater extent.

This simple *induction* is beautifully illustrated and substantiated by demonstrable *facts*, by comparative experiments on soft iron and hard steel; and it was by the same mode of reasoning that I was first led to construct electro-magnets of soft iron,* since which time the practice has been pursued with more than anticipated success.

The facility of polarizing the magnetic matter, or of arranging it into *active polar lines*, by any constant exciting force, appears to be inversely proportional to the retentive quality of the metal on which the process is performed.

The *retention* of magnetic polarity is displayed to the greatest extent by very hard steel. After this the retentive faculty diminishes, with various grades of hardness, down to soft steel; thence by gradations downwards to the softest iron, which exhibits the faculty of retaining magnetic polarity only in a very slight degree indeed. But the facility of magnetizing those bodies, and the extent to which their polarity is exhibited, are in precisely the reverse order.

Now, as the *retention* of polarity appears to result from a want of facility on the part of the metal to re-admit the magnetic matter which the exciting agent has arranged into active *polar lines* on its surface and vicinal medium; and as those metals which display the retentive faculty in the greatest degree also offer the greatest resistance to the formation of those *polar lines*, or to the escape of the magnetic matter from its ferruginous prison, this disposition evinced by the metal, of resisting both the egress and ingress of the magnetic matter, must necessarily arise from a natural tendency which it possesses to refuse the transmission of the magnetic element. Hence those metals which retain magnetic polarity in the highest degree, may be called *inferior* magnetic conductors; and those which retain no traces of polarity after the exciting process has ceased to operate, may be called *superior* magnetic conductors, with as much propriety and for the same reason as similar terms are employed in Electricity.

Under these considerations it will appear that *hard steel* is an exceedingly bad conductor of Magnetism, because it offers a very great resistance to the motion of the magnetic matter. This resistance causes the process of magnetizing to become exceedingly tedious; and with very hard *cast steel* it very seldom terminates successfully, or to the satisfaction of the operator. Hence, in a practical point of view, it is interesting to know that magnets constructed of cast steel should never be harder than the *blue temper*.

* See Transactions of the Society of Arts, &c. vol. xliii. Also these Researches, page 103.

Soft iron, being the best ferruginous conductor of Magnetism, offers a much less resistance to the flow of the magnetic matter than when in any other state. The vigorous *polar magnetic lines* are, therefore, speedily arranged at an extent of concentration never to be accomplished on the surface of the very hard steel.

But the same conducting quality which gives to soft iron a facility of excitation, also gives a facility to the return of the magnetic matter into the metal when the exciting agent is withdrawn, for which reason the retention of polarity displayed by soft iron is exceedingly feeble, and easily deranged.

Hence it appears that, as far as ferruginous bodies are concerned, the vigorous retention of magnetic polarity exhibited by some of them, and the almost total absence of this quality in others, may very easily be explained upon the principles already advanced; and, perhaps, it would only require that we should consider copper and other non-ferruginous metals to be still better magnetic conductors than soft iron, to reconcile the sudden and total disappearance of polarity in them to the same principles, whether the exciting agent be the magnetic or the electric.

I have deflected a magnetic needle by an electric current flowing through an ignited charcoal conductor, as was first shown by the very interesting experiments of Mr. Kemp; but, as we are not aware of the total absence of the magnetic matter in charcoal, the experiment is inconclusive any further than as an interesting fact which has no particular bearing on the present discussion.

The energies of ferruginous electro-magnets are invariably exalted by multiplying, to a certain extent, the number of coils of conducting wire. My large electro-magnet (described in the fourth Memoir, page 116), requires twelve coils to accomplish its maximum of power (400 pounds). The general explanation of this fact is, I believe, that one wire alone is incapable of transmitting or conducting the whole of the electric forces, and therefore a multiplicity of conducting wires becomes necessary in order that the battery may be enabled to give a full and complete display of its electric energies; and, in order to accomplish this object the more completely, the extremities of all the wires are brought as close as possible to the Voltaic plates. The wires of the large American magnet are even soldered to the plates of the battery.

I find, however, that although an addition of coils is attended with an accession of magnetic power until a maximum of polarity is accomplished, it is by no means essential that all those wires arrive immediately at the battery. A single copper wire may intervene between the coils round the iron and the poles of the battery without deteriorating the energies of the magnet, which will still be displayed to a maximum as decidedly as if the whole system of wires were soldered directly to the plates.

My large electro-magnet is still capable of supporting its 400 pounds, notwithstanding the electric force has to traverse six inches of bell-wire *before* it arrives at the coils, and also six inches *more* from its quitting the coils till its arrival at the other

pole of the battery—in all twelve inches of single bell-wire. There is a limit, however, to the dimensions of the intervening wires. If they be too long or too thin, the magnet will not display its maximum of power: with pretty stout bell-wire, and the length not exceeding twelve inches, I always succeed. The battery which I employ is a single pair of metals, sufficiently small to be placed in a pint pot.

This novel and curious fact is one of those which bears directly on the subject in question, and in a theoretical point of view is of a most interesting character. In practice also I find that it is exceedingly useful, giving a facility of manipulation so desirable in the management of very large electro-magnets, but which is not to be expected when all the extremities of the wires arrive immediately at the copper and zinc.

The theory of *polar magnetic lines* which I have advanced requires not two magnetic fluids, nor indeed is it favourable to that doctrine; and if it be not fatal to the circulating currents of Ampere, it will at least require them to be in motion in a great variety of planes which that distinguished philosopher never intended they should pursue. It is possible, however, that electric currents are naturally attended with magnetic polarity, independently of that which has been supposed to be excited in the wire; but it is by no means so probable that the existence of magnetic polarity is universally due to the permanency of electric currents. Electric currents may very possibly, either directly or indirectly, magnetize the terrestrial globe; but we have no reason whatever to believe that such currents are essential to give retention of polarity to steel.

The introduction of *polar magnetic lines* into the theory of Electro-Magnetism would simplify the explanation of the phenomena, and reduce them to the principles of magnetics; and experiments may be shown in both sciences which are favourable to such a conclusion, independently of any consideration that would reconcile to identity the electric and magnetic matter.*

If it can be admitted as an universal maximum in nature, that when one species of matter is impregnated with, contains, or is charged with another, the charged body must necessarily be of a grosser texture than the substance with which it is charged, or that the latter should be more subtle than the former; then it is possible that the magnetic matter, which is the most subtle we are acquainted with in nature, may insinuate itself into the pores of the electric, and the latter become charged with the former as decidedly, under some circumstances, as a piece of iron is naturally charged with them both.

I shall not, however, on the present occasion, advance further into speculative suppositions of this kind, which, however curious they may appear in themselves, are perhaps not of much interest in the present stage of our knowledge of physical operations.

* See Theory of Electro-Magnetism, seventeenth Memoir.

In the positions which I have advanced for exhibiting the *proximate* laws of Magnetic Electricity, I have carefully avoided every consideration that could possibly embarras the mind or prevent them from being understood. They would *virtually*, however, have been but very little affected by taking into account the Magnetism of the metal as an intermediate agent in the process of excitation; but they are much simplified by omitting those *remote* laws, which would be better exhibited separately, and as a distinct class, which may be admitted or rejected at pleasure, without affecting the calculations of the experimenter.

Position 7, with its illustrations, will explain the *apparent* anomalies in the direction of the electric current in wires, when excited at various parts of the surface of the magnet; and will show that, with respect to the exciting *polar magnetic lines*, the direction of the current is constantly the same.

The electrical vortices also, both simple and compound, as I have discovered them to be exhibited by plates and discs, whether rotating on an axis or moving in right lines, may very easily be explained by the same position.

The simple vortex represented in Fig. 11, Plate X. may be regarded as the ring with its exciting *polar lines*, in Figs. 7 and 8, Plate XI. in an inverted order—having the marked ends of the *exciting lines* downwards instead of upwards, which is the case in all the figures of the former plate.

In Fig. 10, Plate X. the ring may be supposed to be advancing with its *external* surface against the exciting *polar magnetic lines*. Hence, the direction of the current in the ring will appear to be reversed—though, with regard to the exciting *lines* which called it forth and gave it motion, the direction remains constantly the same.

The compound vortices in Figs. 3, 4, 8, and 9, Plate X. are easily explained in the same manner, by considering each vortex as a simple ring. In Figs. 3 and 8, Plate X. the *interior* surface of the supposed ring strikes the *magnetic lines* in the vortex on the right-hand side of each figure; but the *exterior* surface of the ring receives the exciting impressions in the vortex represented on the left side of each figure. The compound vortices represented in Figs. 4 and 9 are explained in the same way, by considering them to be receiving the exciting impressions in the contrary order.

By taking advantage of this beautiful law, I have been enabled to exalt the force on the edge of a revolving disc to a consderable extent, as will be shown by the following experiment:—

Experiment 22.*—Let Fig. 14, Plate X. represent a disc of copper, revolving in a vertical plane between the poles of two horse-shoe magnets, situated as in the figure, having the *north pole* of one magnet and the *south pole* of the other on the same side of the disc.

* The Experiments are numbered as a continuation of those in the preceding Memoir.

With this arrangement the electric forces will be distributed as indicated by the small arrows in the interior of the circular plate, when it is rotated in the direction of the large exterior arrows. By this distribution the resulting forces in the upper and lower edge of the plate have the same general direction. In the lower edge the aggregate force or current is in the same direction as that in which the plate revolves, but in the upper edge the aggregate current is in the opposite direction to that of the revolving plate. By reversing the rotatory motion, the whole system of currents become reversed also.

There is a very great advantage by this disposition of the magnets and the copper discs, for not only is the force in the upper and lower edges very much exalted, but by the arrangement of the magnetic poles they very nearly neutralize each other's effects on the needle. To accomplish this point the most decidedly—which is an important consideration in the experiment—the exciting magnets ought, as nearly as they can possibly be procured, to be of the same power.

If, instead of a single disc, the compound disc (described in Experiment 21, in the tenth Memoir) be employed, the excited forces are still more powerful. A large straight needle, placed on a pivot either above or below, with a slight directive tendency in the plane of the plate, will, with a very moderate uniform velocity of the latter, become steadily deflected at right angles to the edge or plane of the revolving disc. Indeed the needle, although at nearly two inches distant from the edge, is very frequently thrown several times round on its pivot by a sudden motion of the disc.

The line of greatest energy in the area of the disc, by the arrangement in Fig. 14, is in that diameter which joins the magnetic poles; and its general tendency is in the direction of the straight arrow, but becomes inverted by inverting the motion of the plate. When one magnet only is employed, as in Figs. 3 and 4, Plate X. the line of greatest energy in the area of the disc is always a curve, unless the plate be very small.

By looking over Mr. Faraday's paper, I find that, amongst other ingenious arrangements, he has also employed a disc of copper in some of his very interesting experiment; but the arrangements with that gentleman's apparatus are very different to those of mine already described.

Mr. Faraday has given to one of his revolving discs the title of "A New Electrical Machine;" and as the deflections which he obtained by this apparatus were by the employment of a delicate multiplying Galvanometer, and those which I have described obtained by a heavy needle, without any multiplying apparatus whatever, it may perhaps be interesting to some readers if we were to bring into one view the results obtained by Mr. Faraday's "*new electrical machine!*" and those which I have shown to be produced by my comparatively old one.

Besides the delicate multiplying Galvanometer which Mr. Faraday has described, he also states that he employed the compound magnet belonging to the Royal Society of London—probably the largest artificial magnet in the world—" composed of about 450 bar-magnets, each fifteen inches long, one inch wide, and half an inch thick, arranged in a box so as to present at one of its extremities two external poles. These poles projected horizontally six inches from the box, and were each twelve inches high and three inches wide. They were nine inches apart; and when a soft iron cylinder, three quarters of an inch in diameter and twelve inches long, was put across from one to the other, it required a force of nearly 100 pounds to break the contact."* (*Philosophical Transactions of the Royal Society of London*, for the year 1832, Part I. page 135.) With this magnetic force, and the assistance of a Galvanometer which multiplied the electric force more than fifty times, " a permanent deflection of the needle of nearly 45° could be sustained."

With my simple electric machine, excited by a magnet of about three pounds weight only, and a needle supported on a pivot, either above or below the edge of the revolving disc, a permanent deflection of more than 40° can be exhibited; and when two such magnets are employed, as in Fig. 14, the needle may be kept steadily deflected at right angles to the plane of the disc.

From this simple statement of facts, we readily perceive that the apparatus of Mr. Faraday exhibits but a very small portion of the excited force in the disc, and leaves in complete obscurity the finest application of that force ever exhibited on the magnetic needle.

The electric force, which may be led or conducted by a wire from a revolving disc, may be very much exalted by taking advantage of the distribution accomplished by the arrangement of magnets exhibited in the following experiment:—

Experiment 23.—Let the disc revolve between the poles of two horse-shoe magnets, having both the *north poles* on one side, and consequently both the *south poles* on the other side of the disc, as in Fig. 15, Plate X. In this case the four systems of forces which flow over the surface of the disc give two resultants in the same diameter. When the disc revolves in the direction of the exterior arrow, those resultant forces will run from between the poles of both magnets towards the centre or axis of motion, where they meet. From the axis of the dics a portion of those forces may be led off by one or more wires at pleasure. The resultant forces will be reversed by reversing the direction of the revolving disc.

* There is a material difference in the proportions of magnitude and power of this magnet and of that which I described in the *Philosophical Magazine* and *Annals*, for March, 1832. Here are 450 bars, which collectively weigh at least 7 cwt. The power of this gigantic magnet on the iron rod is only about one hundred pounds, or not quite 1-7th of its own weight. This force, however, must necessarily be much less than the magnet is capable of exerting on a proper cross piece or lifter; but it is not likely from this fact that it is capable of supporting its own weight. The horse-shoe magnet which I described weighs between nine and ten pounds; and its lifting power equals one hundred and twenty pounds, or about twelve times its own weight. (See fourth Memoir, page 119.)

When four or more magnets are similarly arranged on diameters of the revolving disc, several resultants are driven to or from the axis or centre. By this means the force led off is very much increased. No application of magnets to revolving discs, however, can drive off, through wires, the whole force excited.

Cylinders properly mounted, with respect to the exciting *magnetic lines*, offer a much more efficient apparatus than discs for driving a continuous current through conducting wires. I have made some apparatus upon this principle, but must defer the description till another opportunity.

When a sudden and momentary current is to be exhibited, no mode of excitation hitherto discovered can be employed with greater advantage than that of suddenly making and annihilating a temporary magnet of soft iron, enclosed in a spiral of copper wire—a mode which, I believe, was first introduced by Mr. Faraday in some of his experiments for deflecting the magnetic needle; and which, in the experiments of M. Nobili, and afterwards, in this country, in those of Mr. Saxton and Mr. Forbes, has been so successfully employed in exhibiting the electric spark.

By this mode of excitation, the whole of the exciting *polar magnetic lines* are called forth simultaneously, with a velocity not easily accomplished any other way, and in directions the most suitable to produce the greatest effect.

I have only to add, in this place, that whatever claims may have been made by others to the first discoveries of this branch of science, I apprehend that the experiments and explanations hitherto produced in this series of communications can leave very little difficulty in placing those discoveries in the proper quarter. My vibrating disc (*Philosophical Magazine* and *Annals*, N. S., vol. xi.*) has, I perceive, already been recognised as the first instrument that exhibited phenomena which could not be reconciled to the hypothesis advanced upon the experiments of Arago; and my rotating disc is not only the earliest "machine" of its class, but is at this time the most efficient for the display of magnetic action. The deflections of the needle exhibited by the former apparatus led to the construction and employment of the latter; and although I did not, in my first communication, advance a direct assertion that the excited force in the discs was the *electric*, my statements, to say the least of them, were favourable to the supposition—perhaps as much so as the nature and results of my experiments, and a due regard to propriety would permit. My drawings, however, amply testify that my real views of the character of the force were perfectly correct. It is, however, due to other experimenters that I should state, that I never employed *wires* in my experiments in Magnetic Electricity until I heard of them being employed by Mr. Faraday; and the first time that I witnessed the electric spark, by magnetic excitation, it was shown to me by Mr. Watkins, in his shop at Charing Cross, some considerable time after it had been shown in London by Mr. Saxton, with a similar apparatus.

Woolwich, December, 1832. W. S.

* See Fig. 9, Plate IX.

RESEARCHES IN ELECTRO-DYNAMICS.*

TWELFTH MEMOIR.

Perhaps there is no branch of experimental inquiry, at the present day, more interesting than that of Electro-Dynamics; nor has any department of science been more successfully pursued since the commencement of the present century.

The two leading classes of phenomena exhibited by electric currents are the Chemical and Magnetical, both of which have been discovered within this period, and have become the most important established divisions in the study of Electricity.

The rapid and unprecedented series of successful inquiries which led to the establishment of these important branches of Electricity, had their *origin* in the invention of, and their happy progress *dependent* upon, the Voltaic apparatus, the novel and potent energies of which developed these beautifully interesting and unexplored fields of scientific research.

Notwithstanding, however, the unquestionable supremacy of the Voltaic battery in the production of electric currents, and the splendid discoveries which have been accomplished by its employment, it must ever be acknowledged that, even in its most improved forms, it is a troublesome and expensive apparatus in the process of experiment; and the continual and unavoidable diminishing of its powers, whilst in action, is a defect whose remedy is necessarily precluded by the destructive process required for its excitation.†

The discovery of Magnetic Electricity, however, has led to the construction of novel apparatus, capable of producing continuous electric currents, of undiminished energy, for any length of time they may be required to be in operation, and, at the same time, free from all those defects of the Voltaic battery, and the objections to its employment with which it must ever be attended.

The "Magnetic Electrical Machine," whose exhaustless powers, free of expense and ever ready at command, when brought even to a moderate state of perfection, can hardly fail of becoming a powerful engine of analysis, and a useful and economical implement in the hands of the experimental inquirer: in its present state its powers rival those of moderately-sized batteries, and it is highly probable that they

* This Memoir was read before the Royal Society, June 16th, 1836, but not printed in the Transactions.

† At the time this paper was written, I was not aware of Professor Daniell's improvements in the Voltaic battery; but even now I can see no reason to change my opinion regarding the expense and nuisance attending the employment of Voltaic batteries; nor do I despair of magnetic electrical machines being brought into general use as implements of experimental research.

may be so far exalted by future improvement in the machine, as eventually to supersede the Voltaic apparatus in this branch of physical investigation.

No description that I am aware of, of apparatus of this kind which can properly be called an implement of investigation, has yet found a place in the scientific journals of this or any other country; but, considering the probability of their becoming highly advantageous to the experimental philosopher, when their energies shall have become properly represented and duly appreciated, and in order to call the attention of those who are the most likely to form a proper estimate of these machines and to be benefited by their introduction to experimental research, I have ventured to offer to the notice of the Royal Society a description of two of those forms which I have given to them; and also a brief detail of a few experiments which I have made with them, and which may be considered as expressive of their respective powers. I have also, by way of comparison, detailed a few experiments made with a Voltaic battery, and have ventured to draw a few of the most obvious conclusions with regard to the respective powers of this apparatus and the magnetic electrical machine, and of the advantages likely to be derived from the improvement of the latter.

Description of a Magnetic Electrical Machine, having no Iron Armature.

In Fig. 1, Plate XII. which is a longitudinal section of the apparatus, A A is the edge of a rectangular mahogany base-board, about eighteen inches long from A to A, and ten inches broad; its thickness one inch. B B is the end of another mahogany board, ten inches long from front to rear, and four inches broad from B to B. It is supported over one end of the base-board by two square pieces, one of which is seen at C. The cross-board, B B, thus elevated above the base, forms a stage for the support of the bend, O, of a horse-shoe magnet, O P P, the poles being supported by two pillars, near to the opposite edges of the base-board. A part of one of these pillars is seen at *d*. Near the summit of each pillar is a notch, with an outside shoulder, to prevent the magnet from slipping sideways. A metallic stud, *m*, rises above the stage and directly over the axis of the base-board: this stud carries a steel pivot, on which runs one extremity of the metallic spindle, *i i*. The other extremity of this spindle also runs on a centre pivot, which projects at right angles from the pillar, *e*, the latter being fixed to the base-board.

The spindle, *i i*, passes through the pillar, *f* (which supports the wheel, *g*), and also through the axis of the reel, *k k k k*, to which it is fixed. On the reel is coiled 200 feet of copper wire, about 1-20th of an inch diameter, and covered with stout white sewing silk, to prevent metallic contact in the coil. The spindle, with its reel and

coil, are put into rotatory motion by means of the wheel, *g*, and a band which passes over the pulley, *h*. The reel which holds the wire is made of two thin pieces of deal, of the shape *k k k k*, which form the cheeks, and are kept at about one inch and a quarter apart, and parallel to each other, by two pieces which cross them in such a manner as to leave a deep groove all round between them, for the reception of the wire. (An end view of the reel is seen in Fig. 2.) The extremities of the wire forming the coil, terminate in a discharging arrangement to be described in the sequel.

On the stage, B B, and pillars, *d d*, is placed a compound horse-shoe magnet, composed of four bars of steel, which together weigh about twenty-three pounds. The poles of the magnet are five inches apart, and near to the bend the branches are about six inches apart. The length of the magnet, inside, is about eleven inches; the breadth of each bar about an inch and a half.

When the magnet is placed on its stage, it plane is parallel to the plane of the base-board. The spindle, *i i*, which is also parallel to the axis of the base board, is situated in the axis of the magnet. By this arrangement the coil is made to revolve between the branches of the magnet, and electric currents are excited whilst the coil travels through the magnetic lines, according to the laws of Magnetic Electricity.* The *direction* and energy of the currents will depend upon several circumstances which it will now be necessary to explain.

Excitation of the Electric Fluid in the Coil.

In describing the electric excitation whilst the coil performs its revolutions between the branches of the magnet, it will be necessary to take into consideration the position and direction of the magnetic lines of force through which it has to travel, and which give the exciting impressions. This fortunately is exceedingly simple, and requires but little attention to be understood.

If iron filings be strewed on paper, below which is placed a horse-shoe magnet, whose plane is parallel to the horizon, they will be arranged, by the magnetic force, similar to the arrangement of the fine lines about the magnet in Fig. 3, which may be taken as a pretty exact resemblance of the position of the magnetic force in the plane of the magnet. And if we assume the marked end of the magnet in the figure to correspond with that pole of the compass needle which naturally is directed towards the north, and the unmarked end to correspond with the needle's pole solicited by the south, the strong black lines with cross heads will indicate a similar arrangement of polarity in the iron filings, or in any small pieces of iron situated between the branches of the magnet.

* See eleventh Memoir.

The position of the magnetic lines, between the branches, appears by this arrangement, to be in planes parallel to the plane of the magnet, and at right angles to its axis. (See also Fig. 4, Plate XI.)

A pretty exact representation of the position and direction of those parts of the magnetic force which lie directly above and below the space between the branches, will be obtained by strewing iron filing on paper, below which is placed the poles of a vertical magnet. Fig. 4, Plate XII. will serve to give some idea of such an arrangement, the strong curved lines with cross heads indicating as before the polar arrangement of the ferruginous particles. This figure serves to show that the magnetic force above and below the space between the branches of the steel is exerted in curve lines, and in planes perpendicular to the axis and plane of the magnet.

These are the only parts of the magnetic lines of force through which the coil will have to travel, and consequently those only which will be materially concerned in the excitation.

Let *n s*, *n s*, Fig. 5, denote the direction of the magnetic force between the branches of a horse-shoe magnet, and let *c o a* be an oblong ring of copper wire, also situated between the branches of the magnet, and susceptible of rotatory motion upon a spindle, *p p*, coincident with the axis of the magnet. If now the wire be made to rotate in such a manner that the side, *o a*, moves downwards, and consequently the side, *c a*, upwards, those sides will pass through, and at right angles to, the magnetic lines; and the electric current thus produced will rush through the wire ring in direction of the arrows. But as the wire proceeds in its revolution, the angle between the direction of its motion, and that of the magnetic lines through which it travels, will become less and less (see Fig. 4), on which account, as also in consequence of the diminution of the force above and below the magnet, the excitation will gradually diminish. When the plane of the copper ring has proceeded through a quarter of a circle from its first position, its plane will be at right angles to the plane of the magnet, and its motion in the direction of the magnetic lines: the excitation will then be at a minimum or zero.

As the wire moves on the excitation recommences, but the current is reversed in the ring, because the part, *o a*, of the wire, which moved downwards through the first quadrant, and pressed its foremost surface against the magnetic lines, will now move upward, and receive the exciting impressions on the opposite side. A similar change in these particulars will take place in the part, *c a*, of the wire ring.

Moreover, as the wire proceeds through the second quadrant, the direction of the magnetic lines, and that of the revolving ring, will form a progressively increasing angle, which will become a maximum as the quadrant is being completed. On this account, and also in consequence of the increase of magnetic force, the excitation will be progressively exalted whilst the plane of the ring is describing the second quadrant,

and will be a maximum at the terminal point, or when the plane of the ring and that of the magnet are again coincident.

Whilst the ring revolves through the third quadrant, the circumstances connected with the excitation will be similar to those whilst travelling through the first. The excitation progressively diminishes, and becomes a minimum when the quadrant is completed, at which time the plane of the ring will be at right angles to the plane of the magnet.

When the ring enters the fourth quadrant of revolution, the excitation again recommences, but the direction of the current will be reversed for the same reason that it was reversed when the ring entered the second quadrant. The excitation will be progressively exalted until the plane of the ring be again coincident with the plane of the magnet, at which time the excitation will again be at a maximum.

It is now obvious that, during one entire revolution of the ring, there will be a series of vicissitudes in the degree of excitation, and also in the direction of the current; and a similar series of vicissitudes will attend every succeeding revolution. Twice there will be a maximum, and twice a minimum of excitation; the former taking place when the plane of the ring is coincident with, and the latter when it is at right angles to, the plane of the magnet. The latter position of the ring is that in which the current changes its direction, and may very commodiously be called the *neutral plane*. The plane of the magnet is obviously the *plane of greatest excitation*.

If the revolution of the ring be permitted to commence at the *neutral plane*, the current will not change its direction until the ring has arrived at that place again, or until it has passed through half a revolution; but there will be vicissitudes of energy in that current. The excitation will increase through the first quadrant, but will decrease through the second. And similar vicissitudes of energy will transpire with regard to the reverse current, which will be excited during the progress of the ring through the other half of the circle of revolution.

If now, instead of a single ring, we had an endless coil of wire revolving between the branches of the magnet, every convulsion would receive exciting impressions as decidedly as the single ring; and currents thus produced would flow through the whole length of the wire forming the coil, and undergo all the vicissitudes of energy and direction which have been particularized with respect to the ring.

Hitherto it has been supposed that the ring and the coil form each a complete circle within itself, and consequently the range of the currents limited to those circles, a mode which has been adopted merely to simplify the description of the vicissitudes which the excited currents will undergo during each revolution. But in the construction of the machine, Fig. 1, the ends of the wire, forming the coil, are not soldered together, but are attached to an arrangement of semi-wheels, properly disposed to discharge the excited currents in one and the same direction through any

apparatus which the experimenter may wish to place in the electric circuit. This discharging arrangement is seen at *a b c d*, in Figs. 1 and 6, but will be better understood by describing the latter.

In Fig. 6, which is a bird's-eye view over the end of the spindle, are four semi-wheels, *a b c d*—two of which, *a* and *b*, are soldered to a metallic tube which passes through their centres, and the other two, *c* and *d*, are soldered to another tube, something wider than the former, which also passes through their centres.

The semi-wheels are placed at about a quarter of an inch from each other, on their respective tubes, the smaller of which exactly fits the revolving spindle, *i i*, Fig. 1; and the larger, being lined with an ivory tube for insulation, is also made to fit the spindle, which, passing through both tubes, carries the four semi-wheels, which are by this means caused to make corresponding revolutions with the coil.

The longitudinal opening between the terminal points of the semi-wheels, as seen above and below the spindle, *s*, in Fig. 6, is in the plane of the coil produced; so that the transfer of the current from one pair of semi-wheels to another may always take place at the neutral plane.

One extremity of the wire forming the coil is soldered to the tube carrying the semi-wheels *a* and *b*, and the other extremity to the tube carrying *c* and *d*. The semi-wheels revolve in a glass trough, supported on a pedestal, *t*, as seen in Fig. 1. The trough is divided into three compartments, in which is placed a sufficient quantity of mercury for the periphery of the semi-wheels to run in when on the lower side of the circle of revolution.

The semi-wheels, *c* and *b*, run in the centre compartment, and *a* and *d* in the outer compartments—which latter, however, being connected by a copper staple, the mercury placed in them may be regarded as belonging to the same metallic mass.

If now the spindle, with its coil and appendages, be made to revolve in the direction indicated by the arrow, Fig. 6, the semi-wheels, *a* and *c*, which are in connection with the different ends of the coil of wire, would enter the polar cells of mercury; and would, if the circle were completed by a wire, or any piece of apparatus connecting those cells, convey the electric current from one to the other—the direction of the current depending upon the connections previously made between the extremities of the wire of the coil and the tubes to which the semi-wheels are attached.

When the spindle had made half a revolution, the order and position of the semi-wheels would be reversed; *a* and *c* would then leave the mercury, and *d* and *b* would succeed them in their respective cells. At this time the coil would be in the neutral plane, and as it moved on the current excited in it would flow in the opposite direction to the former; but *b*, which succeeds *c* in the central portion of mercury, is soldered to the other extremity of the wire, and *d*, which succeeds *a*, is also soldered to the opposite extremity of the wire. Hence it is obvious, by this arrangement, that

the changes which take place in the direction of the current in the coil, are accompanied by corresponding changes in the connexions between the extremities of the wire and the mercurial cells. If, therefore, whilst the coil travels through half a revolution, from the *neutral plane* to that plane again, the flow of the current *from* the coil be towards the wheel, *c*, the reverse current, generated through the other half revolution of the coil, would flow towards *b*; and as *c* and *b* succeed each other in the same cell, the mercury there placed would receive both currents from the coil.

A wire or any other conductor joining this mercury, and that placed in either of the outer cells, would convey both currents to the semi-wheels, *a* and *d*, which being attached to the opposite ends of the wire, would, in their turn, dispense the fluid again to the reciprocating currents in the coil. The centre cell which unites the reciprocating currents from the coil, on the one hand, may, for convienience, be called one of the poles of the apparatus; and the outer cells, which are connected, and unite the currents, on the other hand, may be called the other pole.

Apparatus placed in proper connection with these polar cells, would transmit the *united currents* from pole to pole, in one uniform direction, whilst the revolutions of the coil were performed in one and the same direction. If the revolutions of the coil were performed in the opposite direction to the former, the electrical functions of the polar cells would be reversed, and the current through the apparatus necessarily reversed also.

Experiments.

Magnetic.—A Galvanometer, whose coil consists of eighteen feet of copper wire, 1-20th of an inch diameter, covered with sewing silk, and formed into eighteen convolusions, was placed in the circuit of the machine, by proper connections with the polar cells. The magnetic needle belonging to the Galvanometer is four inches long, and weighs 110 grains: it is furnished with an agate cap, and supported on a finely pointed steel pivot in the plane of the coil; and, at the time it was employed in these experiments, it had a directive force, which caused it to vibrate ten times in thirty seconds; or it performed, at the mean rate, one vibration in three seconds.

The coil of the machine was made to revolve with three different velocities—viz. with three revolutions per second; six revolutions per second; and twelve revolutions per second; and with each speed in both directions. The needle's deflection due to the influence of one current in each case, being well acertained before the coil was revolved the other way. The deflection due to the reverse current was also ascertained, and the mean of the two taken as the standard deflection for each rate of motion of the coil.

The diameter of the wheel, *g*, Fig. 1, being twelve times that of the pulley, *h*, the latter, and consequently the coil, revolves with twelve times the angular velocity of the former.

The speed of the wheel was measured by the seconds' pendulum of an Attwood's machine, which was found very convenient because of its addressing its motions both to the eye and the ear. The deflections of the needle, by this means, can be accurately observed, whilst, at the same time, the motion of the wheel can be nicely accommodated to the beats of the pendulum, without the aid of an assistant.

The results of the experiments are arranged in the following table :—

Coil revolved to the	Deflections of the Magnetic Needle due to		
	Three Revolutions per Second.	Six Revolutions per Second.	Twelve Revolutions per Second.
Right,	27°	35°	44°
Left,	23°	30°	40°
Mean,	25°	32·5°	42°

The arcs of deflection exhibited by the above table are those marked by the needle after it had ceased to oscillate, and had become perfectly stationary, and at which it could be kept stationary for any length of time required, by a due and uniform motion of the revolving coil.

The above results present the two following remarkable circumstances :—First, there is an obvious increase of power by an increase of velocity; but there does not appear to be any particular accordance between the velocity of the coil and the arc of the needle's deflection, the ratio of the one being very different to that of the other; neither are the tangents of those arcs proportional to the velocities.

Second, it appears that a steady deflection of the magnetic needle can be maintained by a *variable* electric current, whose vicissitudes of force are uniform and periodical; for it has been shown that the energy of the currents produced by this machine are so exceedingly variable, that it is not constant for any two successive moments during the whole time the machine is in motion.

The fact of the needle's steady deflection by such a variable force, although, I believe, never shown before,* is perhaps no more than what might have been expected, from a consideration that a variable electric current, whose vicissitudes of force are

* It was afterwards ascertained that Professor Cumming had previously shown that a *pulsatory* current produces a steady deflection.

uniform, periodical, and in *rapid succession*, might possibly produce as steady a pause in a deflected magnetic needle, prone to inert repose, as one whose energy is constant and uniform throughout—the mean of the variable being equivalent to a uniform constant electric force in keeping the needle steadily deflected.

The former Galvanometer was removed from the circuit and another introduced. Its coil is similar to that of the former, but its needle is astatic, and suspended by a delicate fibre of raw silk. The compound needle occupied seven seconds in each vibration upon an average of several trials.

The following table exhibits the deflections due to the several velocities of the revolving coil of the machine:—

Coil revolved to the	Deflections of the Magnetic Needle due to		
	Three Revolutions per Second.	Six Revolutions per Second.	Twelve Revolutions per Second.
Right,	60°	70°	82°
Left,	52°	65°	70°
Mean,	56°	67·5°	76°

With the greatest speed which could be given to the coil, the mean deflection was 80°.

When a soft steel needle was placed in a spiral conductor, joining the polar cells, no magnetizing effects were produced.

Chemical.—A piece of unsized white paper was well saturated with a strong solution of hydriodate of potash, and four thicknesses placed on a slip of platinum in connection with the negative polar cell: a platinum wire joined the other polar cell and the uppermost ply of the paper. One turn of the wheel, or twelve revolutions of the coil, determined iodine about the salient* platinum point which rested on the moistened paper.

A strong solution of hydriodate of potash was placed in a small glass, and two terminal metals of platinum wire introduced. Twenty turns of the wheel produced a

* When an electric current traverses a liquid which is connected with the rest of the circuit by metals, the latter may very conveniently be called the "*terminal metals*." That which is connected with the positive pole of the exciting apparatus, and *from* which the electric matter springs into the liquid, may very conveniently be called "the salient terminal metal," or occasionally the "salient terminal" only—the word *metal* being understood; and that which is connected with the negative pole, and through which the electric matter re-enters the exciting apparatus, the "re-entering terminal," or "re-entering metal."

copious decomposition, the iodine being liberated at the salient terminal. When the solution was mixed with a little starch, the liberation of iodine was much more striking.

A piece of unsized white paper was well soaked in a solution of common salt and archil. Two plies of the paper were placed on the terminal platinum foil, and the circuit completed by a platinum wire, one end of which rested on and delivered the current to the upper ply of the paper. Twelve turns of the wheel, or one hundred and forty-four revolutions of the coil, at the rate of twelve revolutions per second, produced a red spot under the salient platinum point. Two hundred and forty revolutions of the coil produced a fine red speck.

Twelve hundred revolutions of the coil produced no such effect when the velocity was reduced to three revolutions per second. This latter result is exceedingly important in the theory of Electro-Chemistry, showing that a certain degree of electrical velocity in the liquid is necessary to accomplish decomposition.

Without altering the last arrangement, a drop of muriatic acid was permitted to redden the paper for some distance about the salient platinum point, and the machine again put into motion. With one hundred revolutions of the coil, a white spot appeared under the salient metal. Two hundred revolutions produced a considerable bleaching effect.

A solution of the muriate of tin was placed in the circuit, being connected with the polar cells by copper wires. The re-entering terminal copper became coated with tin in about six hundred revolutions of the coil, with a velocity of twelve revolutions per second. On reversing the direction of the electric current, and consequently producing a corresponding change in the electric functions of the terminal metals, the tin quitted the wire to which it had been attached by the agency of the former current, and the other terminal (now the re-entering) metal became coated with tin from the solution.

In this experiment it is obvious that, from the first attachment of the tin to the re-entering terminal wire, a Voltaic combination was formed in the solution, the two terminal metals being now tin and copper ; and the current generated by this combintion would be urged in an opposite direction to that of the current from the machine. Notwithstanding, however, this opposing force, the machine current prevailed in carrying on the decomposition, although the re-entering wire was tinned more than half an inch of its length.

A solution of sulphate of copper was placed in the circuit, the terminal metals being platinum wires: no decomposition could be produced with any speed that could be given to the coil.

Whilst contemplating on the result of the last experiment, it occurred to my mind that decomposition might probably be effected by the combined energies of *two* cur-

rents, neither of which *alone* were capable of accomplishing it; and in order to ascertain how far the view which I had thus taken was correct, a feeble Voltaic current was selected to combine with that excited by the machine. The Voltaic combination consisted of a platinum and a copper wire, which were twisted together and their other extremities immersed in the solution of sulphate of copper, and permitted to remain unmolested for half an hour, at the end of which time not a trace of copper could be discerned on the platinum wire, which, in this case, was the re-entering metal in the solution.

The copper and platinum wires were now untwisted from each other and connected with the polar cells of the machine, their other extremities communicating with the liquid sulphate of copper. The Voltaic combination was now as complete as before, but the circuit was lengthened by the two hundred feet of wire in the coil. The wheel was turned in a proper direction to drive the current *from* the copper wire into the solution, and consequently the platinum was thus made the re-entering metal for both currents—viz. that from the machine and that from the Voltaic combination, both of which had to traverse the two hundred feet of wire in the coil, the semi-wheels, the mercury in the cells, the terminal wires, and the sulphate in solution. Decomposition was rapidly produced when the coil moved at the rate of twelve revolutions per second. With about one hundred revolutions of the coil, half an inch of the platinum wire became completely cased with copper. When the wheel was turned in the opposite direction, the copper left the platinum wire.

The first of these results is interesting, as it develops a novel mode of accomplishing electro-chemical decomposition by those currents, which of themselves are insufficient to make the slightest change in the compound.* The second result is also very curious, though perhaps of the same character as the former; for the sulphuric acid alone would exert some slight chemical action on the copper coating of the platinum wire, whilst the current from the machine would assist in its expulsion.

A pleasing and highly-curious variation of the last described experiment is made in the following manner:—When a piece of platinum wire has been coated with copper by the former process, it will answer the purpose of the copper wire in the solution, and will, in combination with a clean platinum wire, form the auxiliary Voltaic combination; so that by removing the copper wire which had previously been used, and putting a clean platinum wire in its place, the latter may be coated with copper by a few turns of the machine in the proper direction. At the same time, that wire which had before been covered with copper will appear quite clean.

* I am well aware that compound Voltaic batteries, and also electric batteries of jars, may easily be considered as producing compound currents; but the currents in each of these cases are from similar sources, and not from dissimilar sources of excitation, as in the experiments here described.

This being done, there is again a Voltaic combination, but in the reverse order to the former. Now change the direction of the wheel's motion, and the machine current will conspire with that of the newly-formed Voltaic pair: the copper again changes places, and that wire which was first coated will now be coated again, and the other as clean as at first. The Voltaic combination is now again reversed: again reverse the direction of the wheel, and another transfer of the copper takes place. These transfers may be made several successive times, but the copper coating becomes less perfect every succeeding transfer, and eventually ceases to appear, showing that the dissolution of the copper, by this means, is decidedly superior to its restoration—a consequence, no doubt, of the machine current conspiring with the action of the acid in accomplishing the solution, on the one hand; whilst, on the other, the Voltaic current, even at first feeble, becomes rapidly more and more so by the copper abandoning the platinum wire to which it had been attached, and eventually counteracted altogether by copper accumulating on the other wire.

It is obvious also from these results, that the action of the acid on the copper is greater than that of the Voltaic pair on it, otherwise there would be a contemporaneous appearance and disappearance of copper on the two wires until both became coated to the same extent, at which time the Voltaic current would cease to exist, and all chemical action terminate. But this is never the case, for the copper invariably disappears from the salient terminal.

Similar phenomena are observed in other metallic solutions. The tin coating of a copper wire, for instance, which has required six hundred revolutions of the coil for its formation, will entirely disappear by one hundred revolutions in the opposite direction; whilst the coating of tin on the other wire is very slight indeed.

A portion of muriatic acid was placed in the circuit, the terminal metals being platina wires: gas was liberated at the re-entering metal, but in so small a quantity that it required very close attention to perceive it.

When one of the terminal metals was copper wire, and consequently a Voltaic combination formed by it and the platinum, gas was liberated at the re-entering platinum by this combination alone, to about the same extent as by the current from the machine; but when the two currents operated at the same time, and conspired with each other, the decomposition was carried on with great promptitude, and hydrogen liberated in abundance at the platinum terminal.

When the machine was turned the opposite way, and the two currents opposed to each other, no decomposition took place. These results are beautiful specimens of electro-chemical action by conspiring electric forces, one of which (the Voltaic) alone is too feeble to maintain a permanent deflection of 3° of the needle of the most delicate Galvanometer. With the latter Galvanometer already described, the permanent deflection with the Voltaic pair alone did not amount to one degree.

Gold and platina, with muriatic acid, form a still feebler Voltaic combination than the latter metal with copper, but even with this trifling auxiliary force, the machine produced a copious flow of gas from the platinum wire. The gold part of this combination was simply a slip of the leaf gold, which was permitted partly to float on the surface of the acid, and partly to hang by the side of the glass vessel containing it. The slip was touched by a point of a platinum wire, the other extremity of which was connected with the salient polar cell of the machine: the re-entering terminal metal was platinum wire. The Voltaic combination was too feeble to produce perceptibly either decomposition of the muriatic acid, or permanent deflection of the Galvanometer needle already described.

Luminous Phenomena.—The spark exhibited by any electrical apparatus can appear under no other circumstance than whilst there is an opening in the metallic part of the circuit; and is necessarily exhibited to the greatest advantage when the machine is undergoing the greatest degree of excitation. Now, with regard to the magnetic electrical machine already described, the opening, if any there be, in the circuit can occur only between the corners of the semi-wheels and the mercury in the polar cells, about the time of their immersion or emersion, or whilst they are relieving each other in the circuit. But this takes place when the plane of the coil is coincident with the *neutral plane*, or when the excitation is zero: hence it is obvious that, under these circumstances, no spark ought to be seen—a fact which is sanctioned by experiment.

In order, therefore, to exhibit the spark by this machine, a metallic point, at nearly right angles to the plane of the coil, is soldered to one end of the coil of wire, and caused to leave the surface of the mercury when the coil is suffering the greatest degree of excitation, or when its plane is passing through the plane of the magnet. The other end of the coil wire terminates with its two semi-wheels, which run in the same mass of mercury as the point moves through; and the spark is seen when the point leaves the surface of the mercury, or at the moment of interruption in the circuit.

Two such points on opposite sides of the axial spindle give two sparks each revolution of the coil; and when the velocity of the latter is considerable, a rapid succession of brilliant sparks is produced.

Effect on Animals.—With the last described arrangement, and a simple contrivance for transferring the current from the point, when leaving the mercury, through a person in connection with the extremities of the coil, a series of shocks are produced which affect the arms to above the elbows. The greater the velocity of the coil, the more frequent and more powerful the shocks are produced.

By means of a pair of medical directors, a well-known electrical apparatus, the shocks may be administered to any part of the body, where access, either directly by the balls, or indirectly by intervening moisture, can be had to the skin.

Recently killed rabbits and other animals are convulsed by the current of this machine in as decided a manner as by the current of a Voltaic battery.

Variation in the structure of the Coil.

When the preceding described results had been fully ascertained by frequent repetitions, the coil of wire was taken off the reel. It was then doubled by bending it in the middle, without breaking, and again coiled on the reel, by laying its strands side by side all the way from the bend in the middle (which now became one end of the coil) to its two extremities, which formed the other end of the coil. The length of the circuit through this coil was consequently one hundred feet, or just half of the length of the circuit in the former coil; having two channels in place of one. The extremities of this coil were properly connected with the discharging system of semi-wheels.

The preceding experiments were repeated with this double coil, and the general results were as follow: the velocities of the coil being the same in both sets of experiments.

The magnetic deflections were nearly the same as before, being a few degrees greater with the single needle, but less with the astatic.

The chemical decomposition less	Than with the single coil.
The shocks very much feebler	
The spark brighter	

Second variation in the structure of the Coil.

The wire was again taken off the reel, and after bending it in the middle was replaced; being now a four-stranded coil. The circuit through the coil was fifty feet, or half of the former; but consisting of four channels for the electric currents to flow through. The ends of the wire being properly connected with the discharging system, the following experiments were made.

Solution of sulphate of copper was placed in the circuit and platina terminal wires: no decomposition could be produced.

With one platina terminal and the other copper, the decomposition was exceedingly feeble; and no complete coating of copper could be given to the platinum terminal. Two hundred revolutions of the wheel, or twenty-four hundred of the coil, at the rate of twelve per second, gave very little effect: merely a yellowish brown tinge was given to the wire.

With muriate of tin in the circuit and copper terminal wires, twelve hundred revolutions of the coil did not produce as much coating of tin on the receiving terminal as six hundred by the double coil. The reverse current did not displace the tin coating in two thousand four hundred revolutions; it perceptibly diminished it, but no more: the other became a little coated.

With muriatic acid and platina terminal wires, no stream of gas could be produced; but the re-entering terminal, after six hundred revolutions of the coil, got covered with minute bubbles, which clung to the wire and could only be perceived by the assistance of a magnifier.

When the salient terminal was copper, and the re-entering one platinum, the gas was liberated from the latter, but in very small quantities when compared with those produced by the preceding forms of the coil.

The spark was not perceptibly different from that produced by the double coil.

The magnetic deflections rather greater than by the double coil.

No shock could be felt from this coil with any velocity which could be given to it.

Third variation in the Structure of the Coil.

The wire being taken off the reel, was again doubled and replaced in a coil of eight strands. The length of the circuit being now reduced to twenty-five feet, the following are the general results obtained from it:—

The magnetic effects not perceptibly different.

The spark also about the same as by the last coil.

No shock could be produced.

With sulphate of copper in the circuit, no decomposition could be effected, even with the aid of one copper terminal. Two thousand four hundred revolutions of the coil were tried with the greatest speed which could be given to it, but without effect.

With muriate of tin and copper terminals, no decomposition: two thousand four hundred revolutions were tried with the greatest speed which could be given to the coil.

With muriatic acid and platinum terminals, not the slightest trace of gas could be produced. When one of the terminals was copper wire, no effect over that of the Voltaic current, by the copper and platinum combination, could be observed

Remarks.

One of the principal points to be gained in bringing the magnetic electrical machine into general use as an instrument of experimental research, was that of combining the reciprocating currents excited in the coil, and causing their energies to conspire in

other parts of the circuit, where various apparatus for experiment could be conveniently introduced; and that this object has been accomplished by the efficacy of the discharging arrangement which has been described appears amply attested, both by the deflections of the magnetic needle and by the exactness of the chemical decompositions; and fortunately similar arrangements, either with or without the employment of mercury, can be applied to any other form of magnetic electrical machines.

In the series of experiments already described, one and the same wire was used in every form given to the coil. The *lengths* of the electric channels decreased in a geometric progression, and the *number* of those channels increased in a similar ratio; so that the product arising from the number into the length gives, in every case, one and the same quantity. But the results of these experiments show that the same length of wire in these different forms of the coil, moving with a constant velocity, and in the same exciting medium, or constant magnetic force, produces very different electric effects.

The electro-magnetic forces appear to be somewhat exalted by multiplying the number of channels and shortening their lengths, whilst the chemical forces, on the other hand, were as evidently diminished by similar arrangements of the coil; and on some compounds were entirely annihilated by the last form given to it, or when the *number* of channels was greatest and their lengths shortest.

The effects on the animal system also lessened with every diminution in the length of the excited circuit, and in this respect corresponded with the chemical effects; the spark was not much different whatever form was given to the coil.

By contemplating electric shocks as the effects of a mechanical action on the animal frame, we obviously arrive at this conclusion, that when the quantity of the electric matter is constant, the violence of the shocks will depend upon the velocity with which that matter is transmitted. This reasoning, on a former occasion, led me to experiments, the results of which were in strict accordance with the conclusion here drawn;* for, by lessening the velocity sufficiently, an infant might be placed in the circuit, without experiencing much inconvenience, of a discharge of a given quantity of fluid, which, if transmitted with great velocity, would knock down the stoutest man.

Now the velocity of the electric matter through any given conductor, from one side to the other of a jar, depends upon the elasticity given to that matter by the exciting process, whatever it may be; and the elasticity depends upon the density, or intensity, as it is frequently called. But, with the Magnetic Electrical Machine, the shocks have obviously some dependence upon the *length* of the excited part of the circuit; hence we may justly conclude that the *velocity* given to the electric matter depends upon the number and magnitude of the exciting impressions given to the coil in a given time.

* Philosophical Magazine, vol. 67, also eighteenth Memoir.

The chemical effects obviously depend upon the same conditions.

If now we consider that the excitation is accomplished by a series of impressions by the magnetic lines of force through which the coil travels, then, if those impressions were of uniform intensity, the velocity of the electric current which they put into motion would be proportional to their number in any given time; and this would be the case whether those impressions were contemplated individually or in uniform groups.

It so happens, however, by this mode of excitation, that the exciting impressions vary in intensity in every part of the circle of revolution of the coil; but, notwithstanding, this circumstance will not affect our reasoning on the total effect produced with different lengths of the excited channel, provided we take, as an unit of excitation, the *aggregate intensity* of the impressing magnetic force exercised during one entire revolution of the coil.

Moreover, it will be convenient, for the purpose of arriving at some definite conclusion in a simple manner, to consider the coil to be revolving with a uniform velocity; and that every convolution of the wire suffers the same degree of excitation, which, perhaps, is not very far from the truth.

Under these considerations, then, it follows that, as in every form of the coil there was precisely the same number of convolutions of the wire, there would also, in every case, be the same quantity of fluid put into motion, when the speed of the coil was constant.

This being the case, let c be the number of convolutions in the first form or single coil; let p represent the impressing magnetic force on each individual convolution during one entire revolution of the coil; and V the velocity of the excited fluid when the speed of the coil is uniform. Then, as the velocity depends upon the impressing or impelling force,

$c\,p = \mathrm{V}$ . . the velocity in the single coil.

$\dfrac{c\,p}{2} = \dfrac{\mathrm{V}}{2}$ . . . . double coil.

$\dfrac{c\,p}{4} = \dfrac{\mathrm{V}}{4}$ . . . . . four stranded coil.

$\dfrac{c\,p}{8} = \dfrac{\mathrm{V}}{8}$ . . . . . eight stranded coil.

and $\dfrac{c\,p}{n} = \dfrac{\mathrm{V}}{n}$ when the wire is in n strands or distinct coils.

Hence, by this mode of reasoning, it appears that the velocity in each individual strand of the wire, which in fact was a distinct coil, ought to be as the number of its convolutions—a conclusion which appears somewhat agreeable to the results of the experiments.

Description of another Magnetic Electrical Machine.

This machine is a modification of one first made by M. Pixii, and, with the exception of the discharging apparatus, Fig. 6, Plate XII. is not very different to that exhibiting in the Adelaide Rooms. A horizontal section of it is seen in Fig. 7, where *m m m* is the magnet already described as belonging to the former machine, Fig. 1; and *n n* is another small magnet, also of four bars, which fits inside of the larger one. Both magnets were made from bars of the same dimensions, and consequently, when fitted together, are of the same thickness.

The piece, *b o a*, is of soft iron, and rotates in front of the magnetic poles, on the spindle, *s s*. The branches, *b* and *a*, are cylindrical, and carry each a coil, *c c*, of three hundred feet of copper wire, covered with silk thread. The iron is made to approach the magnetic poles as closely as possible, without touching, whilst revolving past them on its spindle.

By this means the iron undergoes a series of vicissitudes of magnetic polarity and energy during each entire revolution, and the magnetic force of the iron thus called into action becomes productive of electric currents in the coils.

The method of putting the spindle and appendages into motion is by means of a wheel and band; and as the frame-work is similar to that in Fig. 1, Plate XII. it need not be again described.

Excitations of the Coils.

The plane of the soft iron, as represented in Fig. 7, is coincident with that of the magnet—a position in which the magnetic force of the iron is at a maximum.

If the iron commence a revolution in either direction, the magnetic force of both branches, *a* and *b*, will begin to decline, and the polarity to be less perfectly defined; and will continue to decline until the first quadrant of revolution be completed, or until the plane of the iron has become perpendicular to that of the magnet. In this position, although the magnetic forces be not neutral, they will be at a minimum, and the opposite sides of each branch of the iron will exhibit polarity of different kinds.

As the iron moves on, the extremities, *a* and *b*, will change their polar character, which will progressively become more perfectly defined through the second quadrant of revolution. The magnetic forces will consequently improve until the branches, *a* and *b* be again opposite to the magnetic poles, and have changed places with regard to their first position; and similar vicissitudes of polarity and energy of force will occur during the passage of the iron through the other half circle of revolution.

If now we permit the extremity, *a*, of the soft iron, Fig. 7, to move upwards through a quarter of a circle, or from *a* 1 to *a* 3, in Fig. 8, which is an end view of the circle of revolution, an electric current, whose direction is indicated by the arrows round *a* 2, *a* 3, would be excited in the coil; and notwithstanding the change of polarity that would take place as the iron came into the position, *a* 3, the direction of the current would continue the same until that extremity of the iron had performed half a revolution, and had arrived at the other pole of the magnet—for in whatever direction the magnetic atmosphere of the iron travels through the coil in the first quadrant, it will proceed in the opposite direction on the second: so that, if we conceive that it recedes in the former, it will advance amongst the convolutions in the latter; and as this magnetic atmosphere changes its polarity, with respect to the iron, at the precise moment that it changes its direction of motion through the coil, the direction of the electric current will continue unaltered.

When, however, the branch, *a*, leaves the last approached pole of the magnet, and enters the third quadrant of revolution, there will be a recession of the magnetic atmosphere of the same kind of polarity as that which advanced through the second quadrant; and consequently the new current will be produced in the opposite direction.

This new current will continue during the time the iron is passing through the latter half circle of revolution, as indicated by the arrows round *b* 2, *b* 3, *b* 4, but will again change its direction as the iron passes the pole; so that in each entire revolution of the iron there will be two opposite currents excited in the coil, which will regularly succeed each other as the iron passes the poles of the magnet.

The branch *b*, Fig. 7, of the revolving iron will undergo similar vicissitudes of polarity in an inverted order, so that the currents in its coil will invariably proceed in the opposite direction to those in the other coil; but, by a proper connection of the extremities of the wires, the currents may be made to conspire.

It will now be understood that, as the electric currents change their direction through the coil at the precise time that the iron passes the poles of the magnet, those poles cannot be too well defined, in order that the transit of the iron past them may be as sudden as possible. These views led to the construction of the compound magnet, as represented in Fig. 7.

The discharging apparatus, Fig. 6, is placed on the spindle, Fig. 7, in such manner that the semi-wheels may succeed one another in the polar cells when the changes take place in the direction of the currents, or when the extremities of the iron are crossing the plane of the magnet.

Experiments.

Magnetic.—The following table will show at one view the magnetic deflections with three different velocities of the coils. The Galvanometer is that used in the first described experiments with the other machine, the needle having the same directive force in both sets of experiments:—

Coil revolved to the	Deflections of the Magnetic Needle due to		
	Three Revolutions per Second.	Six Revolutions per Second.	Twelve Revolutions per Second.
Right,	55°	55°	57·5°
Left,	50°	55°	57·5°
Mean,	52·5°	55°	57·5°

With the greatest speed of the wheel, the needle could not be steadily deflected to 60°.

The deflections by this machine do not increase proportionally with those of the other, with the same speed of the coil. The reason appears to be this:—that as the excitation of the coils depends upon the magnetic force developed by the revolving iron, the polarization of the latter is not so complete when the velocity is considerable as when it moves at a slower pace, and this diminution of magnetic force will necessarily abate the excitation whenever the iron moves with great velocity. This circumstance may possibly be a formidable impediment to the improvement of this kind of magnetic electrical machines beyond a certain point; but it cannot possibly be any check whatever to the other kind, where the magnetic force is already formed and permanently situated, and has no dependence upon the revolving part of the machine.

The electric powers exhibited by the first described machine, are certainly much below those exhibited by that with the revolving iron; but it must be remembered that the coil wire of the former is only one-third of the length of that of the latter.

This difference in the length of excited wire, in addition to the difference in the magnetic force employed in the two machines, may possibly account for the difference of excited Electricity. Should this supposition prove a fact, we shall be enabled to carry on the improvements to a very great extent, and in a very simple manner; and large machines may be made of almost any required power.

Chemical.—A solution of hydriodate of potassa and starch placed in a rectangular glass box (such as is used in lectures for exhibiting decompositions, &c.) was brought into the circuit—the terminal metals being slips of platina, with a gauze partition between them. The slightest motion of the wheel gave indications of liberated iodine at the issuing terminal: four or five turns produced a copious liberation, and ten turns were sufficient to produce a dense cloud, when the terminal metal was agitated a little to shake off the iodine with which it was loaded. Not a particle of iodine appeared at the re-entering metal. By reversing the motion of the wheel, iodine was liberated at the other metal. A solution of sulphate of copper placed in the circuit with platinum wire terminals, became partly decomposed by one turn of the wheel: five turns of the wheel liberated as much copper as made the re-entering terminal look like copper wire for half an inch of its length; but none was liberated at the issuing terminal.

A mixture of archil and solution of sulphate of potash was placed in a glass tube, bent into the shape of the letter v, having a platinum wire in each branch, and properly connected with the polar cells of the machine. By a few turns of the wheel the liquor in the branch containing the issuing terminal wire became quite red: that in the other branch a deeper blue than the original liquid.

Strong fuming nitrous acid, diluted with twelve measures of distilled water, was placed in the circuit, having one platinum and one copper terminal. When the latter was made the issuing terminal, the dissolution of the wire was more rapid than by the acid solution alone: when the copper was made the re-entering terminal, the chemical action on the copper was perfectly annihilated.

Twenty feet of thin copper wire, made into a small coil, was made the re-entering terminal in a portion of muriate of tin properly placed in the circuit: the other terminal was a thin platinum wire. The whole twenty feet of copper wire was partially tinned by the machine current in about ten minutes.

A similar coil of copper wire was afterwards tried, with five feet only at a time placed in the muriate. The first five feet soon became perfectly tinned: a second five feet also became tinned in its turn, and so on for five feet at a time till the whole twenty feet were covered with tin.

In this way the twenty feet of wire were better tinned, and in a shorter time, than by introducing the whole at once into the muriate. This result was to be expected, because of the current being too much attenuated to produce rapid decomposition when the whole length of wire was exposed to the muriate.

Distilled water, slightly acidulated by a few drops of sulphuric acid, was placed in the circuit in an apparatus of the usual kind for decomposing water by Voltaic Electricity; the platinum terminals of which being one inch long and a quarter of an inch broad, and three quarters of an inch asunder. A glass tube, filled with the same kind of acidulated water, was inverted over each terminal. The glass tubes are of the same dimensions, each half an inch in diameter, consequently exposing a transverse sectional area of $\frac{11}{56}$ of an inch; then $1 \div \frac{11}{56} = 5\frac{1}{11}$ inches in the length of either tube will be the measure for one cubic inch of capacity.

The coils of the machine were put into motion at the rate of eighteen revolutions per second. At the expiration of twelve minutes $3\frac{1}{2}$ inches of the tube over the re-entering terminal was filled with hyrodrogen, and $1\frac{3}{4}$ inches of the other tube was filled with oxygen: the sum of these is $5\frac{1}{4}$ inches. Hence something more than one cubic inch of gas was liberated in twelve minutes.

From the results of several experiments of this kind, I consider that with these terminals, and water similarly acidulatad, one cubic inch of gas is the average quantity liberated by this machine in every twelve minutes it is kept in motion, the coils making eighteen revolutions per second. With terminals of other dimensions, and with differently acidulated water, the results would be somewhat different.

Having thus ascertained what may be termed the standard power of the machine in decomposing water, comparative experiments were next made with a Voltaic battery.

The battery which was employed is of the Cruikshank form, containing fifty-five three inch plates.* It was charged with rain water mixed with 1-30th of nitro-sulphuric acid—half of which was strong fuming nitrous, the other good sulphuric. The decomposing apparatus, being already supplied with acidulated water of precisely the same character as before, was placed in the circuit the moment the battery was charged, and the following results were noticed:—

For the first five minutes the battery liberated gas much more copiously than the machine, but its powers began to decline very perceptibly at the end of that time. When it had been in action on the water for twelve minutes, the portion of the hydrogen tube filled with gas was measured, and was found to be $3\frac{1}{4}$ inches long.

At the end of the next twelve minutes, the hydrogen column was 4 inches long.

At the end of the third twelve minutes, the hydrogen column was $4\frac{1}{2}$ inches long.

At the end of the fourth twelve minutes, the hydrogen column was $4\frac{7}{10}$ inches long.

At the end of the fifth twelve minutes, the hydrogen column was not quite $4\frac{8}{10}$ inches long.

Now as the hydrogen in one tube is in a constant ratio with the oxygen in the other, the differences in the lengths of the hydrogen columns will be an accurate

* The breadth of the metal exposed to the exciting solution is about two and a half inches, the rest being covered with cement.

measure of the decomposing power of the battery in each interval of twelve minutes. They are as follow :—

	Decomposing power.
First interval	3·25
Second	0·75
Third	0.5
Fourth	0.2
Fifth	0·1

At the end of the fifth interval, or in one hour from the commencement, the decomposing power was nearly extinguished.

By referring the constant power of the machine, or which is the same thing, the constant quantity of gas which it will liberate in any interval of twelve minutes to unity, the quantity of gas in cubic inches liberated by the battery in the successive intervals will stand as below:—

	Quantity of gas in cubic inches.
By the machine in one interval	1·000
By the battery in the first interval	0·956—
second	0·220+
third	0·147+
fourth	0·059—
fifth	0·029+

And by taking the whole hour as the interval of time, the power of the machine to that of the battery in liberating gas is as 5 to 1·411.

The metals of the battery were rolled copper and rolled zinc, and the pairs about the fifth of an inch apart. This latter circumstance tends to shorten the period of action, but exalts the electric powers at the commencement; and rolled zinc has an advantage over cast zinc. A stronger solution would also have exalted the electric powers of the battery for a short time, but then the metals would have been destroyed faster, and the action of shorter duration.

In whatever way we compare the experiments here detailed, it is obvious that the results are favourable to the employment of the machine. Its powers are constantly the same, which is a material advantage both in experimental and theoretical investigation.

The shock from this machine is exceedingly disagreeable, and the sparks which are seen in the polar cells are brilliant, and accompanied by a snapping noise.

Should these facts meet with a favourable reception by the Royal Society, my object will be completely attained. The magnetic electrical machines, when their powers are once known, will soon find their way into the hands of experimental philosophers, and by future improvements may become a more formidable implement of analysis than any hitherto placed at their command.

W. S.

Artillery Place, *Woolwich*, *June* 15*th*, 1836.

APPENDIX A.

During the time I was carrying on the experiments described in this paper, the mercury with its cells were occasionally removed from the machine, and spring *dischargers*, made of brass wire, pressing on the peripheries of the semi-wheels, and lubricated with sweet oil, were made to replace them. A few trials soon convinced me of the advantage likely to be gained by this mode of discharging the currents, but before I had got any series of experiments with this apparatus properly arranged, I was made to understand that my paper would not be permitted to appear in the *Transactions of the Royal Society*—a circumstance which appeared to me no very great inducement to proceed any further with the investigation, at least at that time.

Mr. Christie, one of the Council of the Royal Society, saw some of these experiments, with the spring discharger attached to the machine, at my house, within a week after my paper was read. At that time the machine would liberate one cubic inch of gas, from acidulated water, in eight minutes, showing an increase of power equal to one-third of that which it exerted with the mercurial polar cells.

During the latter part of 1836, I varied the shape and size of the revolving armature, still employing the same six hundred feet of copper wire for the coils. With this new armature the machine liberated one cubic inch of gas in five minutes,* consequently its decomposing power was more than double that which it first exerted with the mercurial cells. During the last winter I again varied the shape and size of the armature, also the length and size of the coil wires, by means of which the decomposing power of the machine was again improved. Since that time I have attached hollow armatures, and also compound armatures, made of bundles of thin iron rods and iron wires of various dimensions, all of which vary the power of the instrument.

* I have not yet seen in London a magnetic electrical machine possessing one-tenth the decomposing power here stated; although that at the Adelaide Gallery is more than five times heavier than mine. Those usually sold in instrument makers' shops do not exhibit one-hundredth part of its power as a decomposing instrument.

I have for some time past been engaged in a series of experiments for ascertaining the comparative decomposing power of this machine in its present form with the powers of the various Voltaic batteries now in general use. In the present instance I have strictly adhered to the paper in its original form, and have neither altered nor added one sentence since it was returned to me from the Royal Society.*

W. S.

APPENDIX B.

The inconvenience of mercury in magnetic electrical machines has been experienced by every one employing them; and in experiments of nice philosophical research it becomes absolutely necessary to dispense with it, and in *every* process it is desirable to employ the whole of the fluid excited. Both these objects have been accomplished by the introduction of discharging pieces, or springs, lubricated by sweet oil whilst pressing upon the metallic arcs attached to the revolving spindle.† The first account of this contrivance was given in a paper of mine which appeared in the *London and Edinburgh Philosophical Magazine*, for 1835, but I had used it for more than a year previously, as will be seen by the following extract:—

" By referring to the number for November last, it will be seen that I had some time previously succeeded in producing Electro-Dynamic phenomena, of various classes, by giving to magnetically excited electric currents one uniform direction through the terminal conducting wires, by means of a certain contrivance which may very properly be called the " *Unio-directive Discharger*," because it has the power of uniting and discharging, in any *one* direction, those currents which, in consequence of the mode of excitation, are originally urged alternately in directions opposite to each other.

" Without some arrangement for this purpose, every magnetic electrical machine, in which coils of wire form the original source, would have remained comparatively useless; and those phenomena, the most interesting in Electro-Dynamics, could never have been produced by the opposing currents, however powerful, rushing from these copious sources of electric action.

* Annals of Electricity, July 1838.

† When oil is not employed, the semi-wheels and spring cut one another, and become rough in a very short time, producing a disagreeable jarring noise; but with oil no noise is heard. The oil is also useful on other accounts. It extends the breadth of the line of contact on both sides of the spring, and thus increases the conducting surface, affording a more copious flow of the electric fluid, as experience amply proves.

" To exhibit the spark, heat wires, or to produce the shock: it matters not in which direction the current flows, nor whether it reciprocates or proceeds in one uniform direction.

" Electro-Magnetic phenomena may also be exhibited by reciprocating currents, or even by opposing currents, provided the force in one direction sufficiently predominates over that in the other; but in the production of chemical decomposition, with exact polar arrangement of the liberated constituents, it requires that the electric currents be not of a reciprocating character.

" It is, moreover, a particular object of the experimenter, in every electro-dynamic process, to avail himself of as much as possible of the excited electric force, and also to prevent, as far as he can, the existence of any counteraction whatever.

" Now, in well-constructed magnetic electrical machines, the reciprocating currents are nearly of equal force, and the predominancy, if any there be, can never be calculated on as a disposable force, as regards either *degree* or *direction.*

" Besides, it would be exceedingly unscientific, in cases where power is wanting, to employ a *part* only when the whole is available; or, as in the present instance,* to employ the *difference* only, instead of the *sum* of the reciprocating electric forces.

" Hence the obvious advantages of the *unio-directive discharger*, which places the whole of the excited force at the disposal of the experimenter, and gives to the magnetic electrical machine a degree of importance which it could never have possessed without it.

" The experimenter also, by this means, may safely confide in his predictions, and vary his exhibitions in any way he pleases as far as the energy of the currents will permit. He is thus relieved from all those corroding apprehensions and mortifying disappointments, which must ever molest his efforts, agonize his feelings, and chill the ardour of his inquiries, whilst operating with an apparatus over the powers of which he has not the slightest control.

" *The following particulars may possibly be interesting to those engaged in Magnetic Electricity:—*

" I produce electric shocks, sparks, steady deflections of the needle, electro-magnetic rotations, &c. and chemical decomposition, with exact polar arrangement of the liberated constituents, by the following forms of magnetic electrical machines.

" 1. By revolving coils of copper wire between the poles of either a horse-shoe or a compound bar magnet, so that the wire may strike, at right angles, the most formidable group of magnetic lines, as shown in the theory of Magnetic Electricity. (See *London and Edinburgh Philosophical Magazine*, vol i. page 31.)†

* This part alluded to machines with a revolving disc and double point discharger, the only kind then known in London.

† This theory is fully explained in the eleventh Memoir.

" With the exception of my revolving discs, described in the *Philosophical Magazine*, for April, 1832, (*Philosophical Magazine* and *Annals*, N.S. vol. xi. page 270)* this is the oldest of my magnetic electrical machines; but, for want of a sufficiently powerful magnet, it was a long time before I had much satisfaction from it. I have more recently been better provided, and I find that it acts well, and appears to me to be better calculated for some points of inquiry than any other form that I have yet seen.

" 2. By revolving coils of wire (having an iron axis or armature) in front of the poles of a horse-shoe magnet. My first revolving armature was simply a straight piece of iron, carrying a coil of wire, and revolving in a horizontal plane above the poles of a vertical electro-magnet. The idea of this form occurred whilst Mr. Watkins was describing to me the well-known apparatus of M. Pixii, an account of which had reached him a short time before. With this form I never did anything more but produce a feeble spark.

" In the autumn of 1833, I first saw, in its present state, the splendid apparatus in the Adelaide Exhibition Rooms, made by Mr. Saxton. This modification of M. Pixii's magnetic electrical machine far exceeding in power my puny arrangement. I have from that time employed bent armature and two coils in the manner of Mr. Saxton.

" 3. Bent armature and coils similar to No. 2. The magnet vertical, and the revolving axis carrying the armature and coils at right angles to the plane of the magnet.

" 4. Similar to No. 3, with the exception of a second piece of armature, with its coils revolving on the same axis on the opposite side of the magnet. The greatest power is obtained when the pieces of armature are placed at right angles to each other.

" 5. By fixed coils on the two branches of a horse-shoe magnet, and a short thick piece of soft iron revolving in front of the poles. This is a very neat form.

" 6. By four cylinders of soft iron, with their coils, permanently fixed to the poles of the magnet, one on each side of each pole. The excitation is carried on by a revolving piece, as in No. 5.

" My *unio-directive discharger*, which can be applied to any of the above forms, is by far the most happy contrivance I have yet hit upon in this class of apparatus. It consists principally of four or more semi-cylindric pieces, properly attached to a revolving spindle.

" The mercury which has hitherto held so distinguished and important a situation in the discharging part of magnetic electrical machines, but which is a complete nuisance to the operator, I have, in most processes, entirely dismissed, by the introduction of my newest forms of discharger."†

W. S.

* See also eleventh Memoir.

† Philosophical Magazine, for September, 1835.

ON ELECTRO-PULSATIONS AND ELECTRO-MOMENTUM.

THIRTEENTH MEMOIR.

PART I.

It is very well known to the readers of the *Philosophical Magazine* that I have long considered electric currents, when transmitted through inferior conductors, between the poles of a Voltaic battery, as the effect of a series of distinct discharges, in such rapid succession as not to be individually distinguished by the senses. Such currents I have called " electro-pulsatory." (See my theory of Magnetic Electricity in the *London and Edinburgh Philosophical Magazine*, vol. ii. page 202. See also the Eleventh Memoir, page 238.)

By following up these views of electro-pulsations, I was, about two years ago, enabled to dispense with all acid or saline liquids, in the employment of Galvanic batteries, for the purpose of Galvanizing, as it is called, either to satisfy the curiosity, or as a medical process ; and my plan, which answers very well, I have found to be productive of a considerable saving in the expense necessarily attendant on the use of Voltaic batteries when excited by acid solutions.

It is well known that a Cruickshank battery of about a hundred pairs will, by employing water alone in the cells, charge to a certain degree of intensity, almost any extent of coated surface of glass that we please ; and that the same degree of charge is given to it by a single contact of the conductors, however short its duration. This being understood, and understanding also that the shock produced by any discharge from a given intensity would be proportionable to the quantity of fluid transmitted in a given time, it was easy to foresee that a series of shocks in rapid succession might be produced by some mechanical contrivance, and that the degree of force might be regulated by varying the extent of coated surface.

My first experiments were made with 150 pairs of three-inch plates, and about seven feet on each side of coated glass ; and the apparatus for producing a rapid succession of shocks was one of Mr. Barlow's stellated electro-magnetic wheels,* which was soldered to an iron spindle and put into a rotatory motion by a wheel and band—the points of the wheel touching in succession a copper spring in connection with a positive surface, and thus producing a discharge at every contact of the wheel and copper spring.

* Historical Sketch, page 27, and Fig. 13, Plate A

When the two surfaces are connected by wires with two basins of salt water, and the hands immersed one in each basin, the effect experienced is precisely that of the discharge of a Voltaic battery. The discharges can be made in such rapid succession as to prevent the sensation of distinct shocks; and if the process were to be concealed, it would require some experience to distinguish between the effects on the animal economy from this apparatus and those from a Voltaic battery charged with acid and water.

My views being so far verified, the next attempt was to simplify the apparatus and make it more portable; and as it was readily seen that, if one hundred pairs would charge glass of considerable thickness, thinner glass might be charged by fewer pairs: this was done, and eventually the glass entirely dismissed, and its place supplied by well-varnished Bristol-board. These boards answer exceedingly well as a reservoir for low intensities: they may be coated to within an inch of the edge all round, and placed upon their edges either on a piece of glass or on a board properly prepared, and arranged to any required extent like the plates of a Voltaic battery; but when considerable intensity is wanted it is better to use thin glass.

From these facts we learn that metallic surfaces of many acres of extent may possibly be charged to a low intensity in the interior of the earth, by having a thin intervening stratum of inferior conducting matter sufficient to insulate from each other their dissimilar electric surfaces.

It may now be understood that the slightest accident which would suddenly break through the insulation, such as the sinking of a mass of metalline matter from one stratum to the other, would cause a sudden rush of an immense ocean of the electric fluid, which might be productive of subterranean lightnings and tremendous explosions sufficient to shake an extensive range of country on every side.

Connected with the preceding facts there are others which may be conveniently mentioned in this place, and which would lead us to similar explanations of the causes of subterraneous convulsions. Electric currents of considerable magnitude, when suddenly checked or diverted to a new channel, produce a momentum not very generally understood, but which I will endeavour to explain:—A coil of copper wire, excited by magnetic action, will become a channel for an electric current, and whilst the whole circuit is metallic, the velocity of that current would be considerably greater than if any, even a small part, of the circuit were of worse conducting materials; and if the current were suddenly transferred from a channel of the former character to one of the latter, by any contrivance whatever, it would meet a resistance on entering the new channel which the momentum it had previously required would have to overcome; and a sudden disturbance of the electric fluid, previously at rest, would take place, and a violent rush of the current would as suddenly follow.

It is in this manner that shocks and sparks are produced by magnetic electric machines, where the current, previously in rapid motion, is suddenly transferred to a new channel of inferior conducting matter; and all the fluid in the revolving coil rushes through a person properly situated for the new route, and who experiences the electric shock, or else through a thin stratum of air at an interruption in the metallic circuit where the spark is produced.

These then are some of the effects of electric currents, or of the momentum of the electric fluid in a state of motion, after the exciting cause is entirely cut off. The shock thus produced may very conveniently be compared to the blow given by Montgolfier's hydraulic ram. Electro-momenta may be produced by any mode of excitation whatever, and the effects will be proportional to the velocity and quantity of the electric fluid first put in motion; and the length of the original channel is also to be taken into account. If then electro-momenta, capable of producing violent shocks and vivid sparks, can be produced by a few hundreds of feet of thin copper wire, what is it that might not be expected from the electro-momenta of nature, arising from currents of many miles in extent, kept in motion either by heat, saline solutions, or by other causes, amongst the metalline strata below the surface of the earth? A sudden disruption in the circuit would insure the blow, and an earthquake might be the result.

W. S.

Artillery Place, Woolwich, July 4th, 1836.

P.S.—Since the above was originally published, I have formed an electrical battery of coated glass, consisting of ten rectangular pieces of common thin window glass, each of which is twelve inches by eight and a half; hence each surface is precisely 102 square inches, the double of which gives 204 square inches for each piece. Therefore the ten pieces expose a surface of 2040 square inches of glass, the coated part of which is about 1717 square inches, being little short of twelve square feet.

The coating of the glass consists of twenty rectangular pieces of tinfoil, each about ten and a half inches by seven and three quarters. Each piece of foil, when fixed on one surface of the glass, reaches to one of the longer edges, which it just covers, leaving a naked margin round the other three edges of that surface. The opposite surface is coated in the same manner, but the metallic foil now reaches the opposite edge of the glass, and also just covers that edge, leaving a margin of glass round the other three edges of the foil. In order to combine these coated panes of glass so as to form an electric battery, they are arranged in a grooved mahogany box, in precisely the same manner as are the plates of a Cruickshank's Voltaic battery, but not fixed in the grooves. The bottom part of the inside of the box is well covered with tinfoil, on which the lower coated edges of the glass plates rests, and are in good metallic contact.

Fig. 1, Plate XIII. represents a vertical section of the battery and its discharging apparatus. B B B is the mahogany box, with six of its glass panes, 1, 2, 3, 4, 5, 6, as seen edgewise. On the *right hand* of the edge of each pane is a dotted line, which reaches to the top but not to the bottom; and on the left side of each pane is a similar line, which reaches to the bottom but not to the top. These dotted lines are intended to represent the metallic coatings, from which it will be understood that the whole of those on the left hand are in connection with each other by means of the metallic lining at the bottom of the box; and that those on the right hand can at any time be united by a metallic rod being laid across the top of the series. This rod is not shown in the figure, to prevent confusion in the explanation.

The charging of these coated panes is accomplished by connecting the lining of the box with the negative pole of a Voltaic battery, by means of the wire, N W O; and the positive pole with the wire, P, which is in connection with the rod that lies across the upper coated edges of the glass.

The apparatus for producing the discharges is an oblique-toothed metallic wheel, H, and a metallic spring bar, S. The former is supported by two brass pillars, and turned on its axle by means of a handle; and the spring bar, S, is supported on a glass pillar, and connected with the *positive* surface of the glass, as represented by the figure. From one of the brass pillars of the wheel proceeds a copper wire, which terminates in a basin, A, partly filled with salt water; and another basin, W, also containing salt water, is connected with the *negative* surface of the glass, by means of the copper wire, W O. If now a hand be placed in each basin of water, the negative surface becomes united with the brass wheel, H; but the circuit is prolonged no farther, until, by turning the wheel, a tooth is brought into contact with the spring, S. By this contact, however, the circuit becomes closed, and the consequent electric discharge produces a slight shock. If the wheel be kept in motion, at a moderate speed, a succession of alternate charges and discharges of the coated glass takes place; and a corresponding series of shocks is the consequence which, though feeble individually, produce an extraordinary accumulation of effect. No shock is produced independently of the glass battery.

W. S.

ON THE ELECTRIC SHOCK FROM A SINGLE PAIR OF VOLTAIC PLATES.

THIRTEENTH MEMOIR.

PART II.

A week or two before the Bristol meeting of the British Association, I was particularly gratified by an intimation which I had of an experiment made by Professor Dr. Henry, of the New Jersey College, Princeton. My informant was a Mr. Peaboddy, a scientific American gentleman, whom I accidentally met with in the Adelaide Gallery of Practical Science. The experiment, as described to me, was to convert *quantity* of the electric fluid into *intensity*, by means of a single Voltaic pair—the indication of intensity being that of producing a shock. Whether this be Professor Henry's real meaning or not, I have no further means of ascertaining. It occurred to me at the time, from what I could learn from Mr. Peaboddy's description of the apparatus by which Professor Henry had made the experiment, that the effect was due to the momentum of the fluid put into motion—not, perhaps, from its having a great degree of tension in the Voltaic circuit, but from its being transferred suddenly to a new channel, in precisely the same way as shocks are produced by a magnetic electrical machine. One of the principal circumstances to be attended to, in order to produce a shock, as it appeared to me, is that of having a sufficient extent of circuit; for whatever be the mode of excitation, the whole of the fluid belonging to the conducting wire will be put into motion; and if it moves with a sufficient celerity, the momentum it acquires will enable it to overcome the resistance of a worse conductor by suddenly transferring it from the former to the latter; and by this means it might, perhaps, be transmitted through an inferior conductor, which, without such momentum, it could not penetrate.

I am not aware from what train of reasoning Professor Henry has been led to construct an instrument which will produce shocks by one pair of plates; but as I understand that the ingenious Professor has not yet published his invention, and as it is probable that Mr. Peaboddy may have told many other persons of the fact, I consider that I cannot render Professor Henry a better service at this time than by securing for him the credit of his experiment in the pages of these Annals.* I must observe, however, that as I am unable to describe the exact mode by which the experiment

* Annals of Electricity, &c.

was made at the New Jersey College, I cannot do any more at present than describe that by which I have repeated it.

In Fig. 2, Plate XIII. A B represent two coils of copper wire, each containing about 300 feet, and well covered with sewing silk. The inner ends of the wires forming the coils, are joined together by solder at S; and on the upper side of the joining is soldered a disc of copper, whose upper surface is quite bright. To the outermost extremity of the wire belonging to B is soldered a cylinder, N, of brass; and near to the outermost extremity of that belonging to A is soldered another brass handle, P. The end, S, of the wire, S Z, merely rests on the plate, S, and the other end, Z, is in connection with the zinc side of the Voltaic pair, and C, being connected with the copper, the apparatus is complete.

The coils which I have employed are some of those belonging to my magnetic electric machines; and the battery one of my cylindrical pots, holding about a quart of liquid.

Suppose, now, a person with moistened hands takes hold of the handles, N P, one in each hand, then the circuit would be made up of two channels—one very good conducting channel from the copper, C, through the coil, A, and round by S to the zinc, Z; the other from C to P, thence through the person connected to the handle, N; thence through the coil, B, along and by S to Z. This latter channel is rendered a bad conductor because of the person being placed between P and N; and perhaps, by this intervention, the whole current travels by the former route in the direction of the arrows leading from C, and in the direction, S Z. Now it is obvious that the whole of the fluid belonging to the coil, A, is kept in motion by the action of the battery, whilst that in B is very little, if at all, disturbed. Let now the end, S, of the wire, S Z, be suddenly lifted off the plate; the fluid which is in motion in the coil, A, can no longer travel towards Z, because of the interruption at S; but as it has access to the coil, B, it will, by its momentum, disturb all the fluid in that coil, and drive suddenly against that in the person situated in the circuit between the two handles, N P, who will, in consequence, experience a shock. Let now the position of the Voltaic plates be changed, so that the current will flow in the reverse order, or from Z to C in the figure. In this case, as well as in the former, the superior conducting circuit would be through the coil, A, whilst the fluid in B would remain nearly at rest. Again open the circuit at S: the fluid in motion in A, now rushing in the direction, P, would drive against that in the person between P and N. As the fluid from the coil, A, pressed in this direction, it would be followed by that in the coil, B, which would facilitate the disturbing of that in the bad conductor, and conduce to the production of the shock.

If this explanation be admissible, it is easy to perceive that the shock would be produced in whichever direction the first current flowed through the coil, A—although, perhaps, it might not appear so obvious by what means the coil, B, contributes to the

shock. I do not, therefore, give it with a view of supporting it, but as one which occurred to me at the time I was repeating Professor Henry's experiment. Fig. 2 is very unlike the apparatus which Mr. Peaboddy described, but I believe it is the same in principle. Professor Henry's apparatus consists of a long strip of sheet copper (I was not told its length or breadth), formed into one coil, in the manner of a watch spring, having one of its extremities in connection with one of the plates of a large calorimoter, and about half way between the extremities of the copper strip is connected the other Voltaic plate; and the person who experiences the shock is placed in connection with the two extremities of the coil in precisely the same manner as in Fig. 2. I did not feel disposed, however, to cut two or three sheets of copper into strips for that purpose, and having several coils of wire at hand, I considered that they, perhaps, might answer the purpose quite as well; and the arrangement I made was precisely that shown in Fig. 2. A shock is produced every time the contact is broken at S, but none is given on completing it again. This is just what happens with the magnetic electrical machine. If the upper side of the plate, S, be made rough with a file, and the point of the upper wire be drawn over it, a series of shocks is produced in rapid succession. By applying a small rough edged wheel to shake the end, S, of the wire, Z S, in such a manner as to permit it to touch and untouch the plate, or break and make the contact in rapid alternation, the shocks are converted into a disagreeable pulsatory stream.

Having thus satisfied myself as far as my information of Dr. Henry's experiment had conducted me, I became desirous of ascertaining other particulars concerning the arrangement of the apparatus; but at that time I had no opportunity of carrying on my inquiries in the manner I wished, nor was it till the 23rd of the present month that I could find time for that purpose.

My first object was to ascertain whether or not the coil, B, was conducive to the shock; and, after many trials, it was found to lessen rather than increase the intensity.

In order to explain the manner by which I ascertained this fact, the reader must imagine the handle, N, Fig. 2, to be connected with the coil A, at S, and the coil B taken away. The first, or principal current, by this arrangement, is through the coil A, from C to Z, as decidedly as before; but when the connection was broken at S, the current from A rushed immediately towards N, and consequently to the person placed between N and P. This variation of the experiment proves the coil, B, to be of no use in the manner it was before used; or, indeed, something worse than useless, because the shock was more powerful without it. Although I had tried both arrangements several times over, yet, as I was employing rather an active battery, I still suspected that I might possibly fall into error in consequence of the difficulty of keeping such a battery in uniform action. This thought led me to try weaker acid solutions, and eventually I resorted to salt and water for the exciting liquid, and began with a new

pair of metals. Still, however, I found that the coil, A, alone did better than with B attached.

It very often happens in experimental researches that we are led to digress from the path of inquiry we have previously marked for the pursuit, by the appearance of some unexpected fact which obtrudes on our notice; and it was from discovering that salt water was a sufficient exciter to produce very smart shocks in this instance, that I was led completely from my principal line of pursuit to inquire how far the experiment would permit the battery to be diminished in size. It will be of no interest to many readers concerning the route of my experiments on this point: I will tell them at once that I soon reduced it to the size of a lady's thimble, and still produced considerable shocks. Eventually I tried two wires, one copper the other zinc, about one-twelfth of an inch thick, and immersed to about an inch deep in diluted nitrous acid. Even with this miniature battery, smart shocks were produced; and I have no doubt that the smallest fragments of metal might be made to produce sensible shocks.

Having ascertained this curious fact, I returned to the original line of inquiry, and endeavoured to ascertain how far the length of the wire was concerned. The coils were now connected by solder at S, and the wire, S Z, soldered to the outermost end of the wire belonging to B, near to the lower arrow beside the handle, N; and a contrivance for opening and closing the circuit was placed between C and the joining of that wire with the handle, P, so that when the Voltaic circuit was broken there would still be a metallic connection from P to N.

With this arrangement, the principal circuit would be from C to Z, through both coils, six hundred feet of wire; and when the circuit was broken about the arrow at C, the fluid now in motion in both coils would produce a disturbance of the fluid in the person joining N and P, and a shock would be experienced, which the experiment proved. The shock, however, by this arrangement was not so great as by one coil only. The coils were next tried side by side, so as to form a double channel of three hundred feet. Very little was gained by this arrangement. The effect in some trials appeared rather greater, in others less than by one coil only.

Another coil was now tried: the wire was rather stouter than the former, and the reel was of wood. The reels of the coils before used were of metal, which it was thought might possibly affect the results. The wire on the wooden reel was the same length as on either of the others—viz, three hundred feet. The shocks were equal, if not superior, to those with one of the other coils.

Remarks.

The shock is produced entirely without, or exterior to, the Voltaic circuit.

The spark is much brighter than when no coil is in the circuit.

The shock is never produced only at the moment of opening the Voltaic circuit, none being experienced when the contact is made.

If there be any spark whilst making the contact, or closing the circuit, it is exceedingly feeble when compared to that seen when the circuit is opening.

About 300 feet of copper wire has, by these experiments, answered better than 600 feet in sequence.

With 50 feet no shock could be perceived.

A single wire, 300 feet long, has answered as well, or better, than two such wires, forming a double conductor.

Copper wire, one-twentieth of an inch diameter, answers better than thinner wire.

A pair of cylinders, one of copper the other of zinc, which will enter a pint porcelain jar, and excited with cold salt water, is quite a sufficient Voltaic power to produce very smart shocks.

With acid and water, and a Voltaic pair of thin copper and zinc wires, one inch long, shocks may be produced.

With regard to the length and thickness of the conducting wire, it is considered that much may possibly depend upon the extent and nature of excitement of the Voltaic surfaces: no batteries were used but such as have been described.

W. S.

Artillery Place, Woolwich, Sept. 24th, 1836.

P.S.—Since the preceding paper was written and sent to press it occurred to me that I had neglected a certain point in the inquiry, which might, perhaps, affect the results very materially. I thought that if the coils had had each a nucleus of soft iron in it, the Magnetism which would be brought into play might possibly either increase or diminish the shock. Indeed, I had no idea what might happen, which was quite enough to begin with. A few experiments, however, soon convinced me that the iron I employed had not much influence, either the one way or the other; and, if my curiosity could have rested here, I might have saved myself a great deal of trouble by terminating my experiments at this point. I could not, however, get entirely rid of the Magnetism which thus entered my mind, and a train of ideas which had long ago been formed now flashed with redoubled force into my recollection.

It has been my opinion for many years, that in all cases of electro-magnetic action, where a needle or other ferruginous body is operated on, there is an intermediate agent; and that the deflections, &c. are not the immediate effects of the electric matter, but are secondary effects—the primary effects being the magnetizing or polarizing of the magnetic matter in the conductors, and perhaps of that also of the surrounding medium. It is on this principle that I have always in my lectures explained the attractions and repulsions of parallel conducting wires—phenomena first shown by

the late M. Ampere;* and I have stated this to be my opinion in my paper on the theory of Magnetic Electricity, published in the 81st vol. of the *Philosophical Magazine, &c.*†

Although these views of electro-magnetic action have so long occupied my attention, I have never had sufficient spare time from other pursuits to arrange my ideas and explain facts by means of this hypothesis. But now, having the implements in my hands, and a point of determination regarding the correctness or incorrectness of my views appearing just within my reach, I determined to put the question immediately to the test of experiment. But I must explain a little farther by what reasoning I was induced to undertake a whole day's labour to satisfy myself on this one point; and I am very sorry the drawings are all gone to press, otherwise I might have drawn a figure that would have facilitated the explanation very much. Fig. 3, Plate XIII. must answer as a substitute.‡

If the tangential lines in that figure be virtual magnets, they ought to assist each other in their development when they are placed sufficiently near to each other. Imagine that c and z are the ends of a bent conducting wire, whose bend, or curved part, is behind the paper. The magnetic force in every transverse section would then be represented by those tangential lines, or by lines similarly situated. Let the spectator now imagine that the end, z, of the wire is continued to a sufficient length to turn upwards over the end, c, and proceed to behind the paper, turn round the former bend, and arrive again below the section, z. The wire would now form a coil of two convolutions.

Let now sections of this last convolution be exposed to view, one above c, the other below z, as represented by c' and z', Fig. 4. The tangential lines in these sections, and of those of every succeeding convolution, would observe the same arrangement as those in the figure; but the tangential lines on the *inner* side of the last convolution will be in the reverse order to those on the *outermost* side of the first, on which they are superposed. The north poles of one convolution will be opposite to the south poles of the other, between the wires, in every section that can be imagined; and the same will happen between every two convolutions throughout the coil, whatever may be its extent. And if sections be drawn on both sides of c and z, as represented by Fig. 5, it will be observed that the arrangement is precisely of the same character, and that south and north poles are presented to each other in every part of the coil.

Under these circumstances then it would appear that, when once the magnetic arrangement is accomplished in the coil wire, the polar attractions of the elementary magnets will tend, in some degree, to keep them thus arranged; by which means that part of the electric force, which is equivalent to the aggregate force of all the multi-

* See Historical Sketch, page 12, and Plate A, Fig. 9.

† See also the eleventh Memoir, page 246.

‡ Fig. 3 is similar to that referred to in the original.

tudes of magnetic attractions, will be relieved from that duty, and will join and assist the remaining electric force in overcoming other resistances which it had to contend with—for whatever be the nature of the resistance that an electric current has to overcome, it tends more or less to retard its motion. Magnetism is not brought into play in the conducting wire, nor in iron in its neighbourhood, without an expenditure of some part of the electric force ; and consequently the electric force lost by keeping a straight wire in a state of magnetic polarity, would be in some ratio with the extent of Magnetism displayed. But this would not be the case if the wires were formed into a close packed coil. The polar magnetic energies, once developed, would be arranged in the best possible order for mutual polar attraction, and the magnetic resistance thereby very much abated. The coil wire would thus be rendered a better conductor, and would permit the current to flow more rapidly than if no such relief had been afforded ; and consequently the momentum ought to be greater by the conductor being in a close coil than when straight or even loosely folded in a skein ; and if the shocks depend upon the momentum, they ought to be *more intense* by the conductor being in a compact coil.

Such were the results of my reasoning on this point, and nothing less than an ardent hope of realizing them experimentally, could have induced me to undertake the labour which I then saw placed in the way to truth.

I had only fifty feet of silked wire that was uncoiled : this was placed in the Voltaic circuit, but no shocks could be perceived. The wire was next coiled closely round a cylinder of wood, and again placed in the circuit, but still no shocks were discovered. I was not much surprised at these results, because I considered that the wire was probably too short ; I therefore determined on uncoiling one of my 300 feet wires which had previously been ascertained to be conducive to the shock. I had three of these coils.

This was done, and the wires, loosely hung round a chair back, was placed in the circuit : no shock could be produced.

The wire again coiled on the reel, and again placed in the circuit : the shocks were as powerful as at first.

Another wire was uncoiled and placed in the circuit : no shock could be produced. The wire was again coiled : the shock as great as at first.

The third wire was again uncoiled and placed in the circuit : no shock again.

The wire was again coiled, and again placed in the circuit : the shock was as great as at first.

These results were exceedingly gratifying to me, not so much at having satisfied my curiosity, as by the success which had attended my labours in bringing to light a novel principle, which, though long perceived by the mind, had in every other respect been permitted to remain in concealment, and till this day unregistered in the pages of philosophy.*

* It will be seen by the note appended to the next Memoir, that I had been anticipated in the development of this fact.

I am well aware that other facts may appear necessary to establish the correctness of the hypothesis which led me to this discovery, and doubts may possibly arise respecting the increase of velocity of the current, upon the principles of reasoning which conducted me to that conclusion. Time alone must decide these matters. I have not one moment left to give it another consideration. It is now eleven o'clock at night, and I have been at work almost without intermission since seven in the morning, and every other part of this number* is now in the press. I will, therefore, content myself for the present with registering the fact, leaving my hypothesis to the decision of others.

It just occurs to me whilst writing, and looking at the rough sketches which I have drawn to assist the arrangement of my ideas, that it is possible an impulse may be given to the current by the sudden transiliancy of the magnetic tangential lines from a state of vigorous polarity to that of complete annihilation, at the precise moment the battery action is cut off.

W. S.

Artillery Place, Woolwich, Sept. 28th, 1836.

AN EXPERIMENTAL INVESTIGATION OF THE LAWS WHICH GOVERN THE PRODUCTION OF ELECTRIC SHOCKS, &c. FROM A SINGLE VOLTAIC PAIR OF METALS.†

FOURTEENTH MEMOIR.

From the disagreement of results in some of the experiments detailed by Dr. Faraday, and those described in the preceding Memoir, I have been induced to look again at what I had done, and to repeat those experiments in which the difference in our results was most conspicuous.

In comparing our experiments, however, I have been enabled to perceive that, in fact, they were not exactly of the same character, but differ essentially from each

* Annals of Electricity.

† It has already been stated in Abstract 32, page 45, that my first series of researches (described in the preceding Memoir) were carried on without any knowledge whatever that any inquiries of the kind had been pursued in this country; and that it was not till after that series had been published in the *Annals of Electricity* that I became acquainted with Dr. Faraday's *Ninth Series of Researches*, which is entirely devoted to the same subject.

It appears that this particular topic originated in a curious experiment first made by Mr. Jenkin, in the year 1834 The apparatus employed by that gentleman consisted of a long cylindrical helix of copper wire, and an iron rod, about two feet

other by reason of the difference of the apparatus we employed. I have already noticed in the preface to Dr. Faraday's paper,* that I then suspected that the disagreement of the results of our experiments, when iron was employed, was probably owing to the fashion of our coils, which I now find has a considerable influence over the results. I have also been led to the discovery of other circumstances which, by modifying the phenomena, become of considerable interest in the theory of the action.

The iron which I first employed was the rotating armature of a magnetic electrical machine, and the coil of 300 feet of copper wire was placed on one of its branches, and when carrying an electric current the armature was converted into a temporary horse-shoe magnet. By this application of the iron, I could perceive no increase in the power of the shocks, they being of equal intensity when the iron was not present. This experiment I have repeated, and the results are similar to those I first noticed.

I next instituted a new series of experiments with coils and iron of other fashions.

Experiment 1.—60 feet of copper wire, one-twentieth of an inch diameter, and well covered with silk, were made into a helix on a card-board tube, which would just admit a cylindric iron bar of half an inch diameter. The helix was nine inches long, and consisted of three strata of close packed coils from end to end. The iron bar was twelve inches long. The metals forming the battery were cylinders of copper and

long and half an inch diameter, which could be placed in the hollow axis of the helix, or removed from it at pleasure. The helix itself consisted of three small wires laid parallel to one another, as so many strands of one compound wire, which terminated at each end by one stout copper wire. To the end of each thick wire was soldered a copper cylinder, to be grasped in the hands of any person operated on. The battery employed was a single pair of Voltaic plates, the zinc surface exposing about three square feet, having the copper opposite both sides. "On holding the two copper handles tightly in the hands, previously moistened with brine, and then alternately *making* and *breaking* the contact of the ends of the helix with the electromoter (Voltaic pair), there was a considerable electric shock felt in the latter case—*i. e.* on breaking contact, provided the iron rod were in the helix—but none either on making or breaking contact when the latter was away." (*Philosophical Magazine*, for May, 1834.)

Dr. Faraday having received the above information from Mr. Jenkin, immediately began a series of experimental inquiries into the subject, the first of which were described in a letter to Mr. Richard Phillips, which was published in the *Philosophical Magazine*, for November, 1834. In the December number of the same journal appeared another letter to Mr. Phillips, in which Dr. Faraday corrects some mistakes made in the former one; and in his *Ninth Series of Researches*, read before the Royal Society, January 29th, 1835, the inquiry is extended to a great length, and several novel facts developed, the most interesting of which are *secondary electric currents*. If, for instance, two wires, covered with thread, be laid close together and parallel to each other, and through one of them be transmitted an electric current from a single pair of Voltaic plates, a *second* current will be produced in the other wire at the precise moment the original current is cut off; and a similar *secondary* current, but in the opposite direction to the former one, will be produced in the unconnected wire at the precise moment that the battery current *re-commences*—*i. e.* on closing the circuit.

There being several experiments instituted in the researches detailed in the thirteenth Memoir, differing from those made by Dr. Faraday, and our views respecting the exciting cause, being also different, I have not thought it proper to alter the original form in which the results of my first inquiries appeared. The present Memoir advances the inquiry another stage, and the fifteenth and sixteenth Memoirs bring it to a position beyond which it has not hitherto been pursued.

* See Abstract 32, page 45.

zinc, the former four inches in diameter, the latter about three, and eight inches high; they were placed in a porcelain jar, so that the exciting liquid could have access to all the surfaces. The liquid was salt and water, cold. With this battery and the helix in the circuit, I could perceive no shocks either with or without the iron. The sparks, when shown at the surface of mercury, were alike in both cases; so were the scintillations, by running a fine iron wire termination over the rough surface of a properly connected plate.

Experiment 2.—The helix being removed, the wires of an electro-magnet were next placed in the circuit. These wires were 12 in number, and the average length of each about 40 feet. They formed 12 distinct coils, one above another, round an iron bar, bent into the form of a horse-shoe magnet, whose branches have scarcely any curvature from the central bend to their extremities, which are about an inch-and-a-half asunder. The bar weighs sixteen pounds. No shocks could be obtained by this arrangement, although the magnetic power at the poles amounted to about 30 pounds. The spark was small.

Experiment 3.—The electro-magnet being removed, a coil of 300 feet of copper wire was introduced. The wire of this coil was about the same diameter as that of the coil in Experiment 1, and well covered with silk. The coil was formed on a wooden bobbin, whose inner diameter was about two inches and the length the same. The shocks were pretty strong: iron wire scintillated; the spark bright.

Experiment 4.—The same battery metals were again employed. The liquid was salt and water, nearly boiling hot. When the first mentioned coil of 60 feet of wire (Experiment 1) was placed in the circuit, no shocks could be obtained. When the iron cylinder was placed in the coil, slight tinglings were felt in the fingers when dipped into salt water in two small jars properly connected.* The spark not perceptibly different with or without them.

Experiment 5.—When the wires of the large electro-magnet (Experiment 2) formed the circuit of this hot battery, no shock could be obtained; but a bright spark was observed whenever contact was broken. The magnet in this case would lift 70 pounds.

Experiment 6.—When the short thick coil of 300 feet of wire (Experiment 3) formed the circuit, the shocks were more powerful than with the cold solution, and the sparks and scintillations much brighter.

Experiment 7.—A smaller battery was now used, rendered very active by nitrous acid and water. With the coil of 60 feet of wire (Experiment 1) and no iron, no

* When the power is very feeble, it is useful to know that the sensation is best obtained in the little finger, when immersed *alone* in one of the portions of salt water, and the other finger and thumb in the other portion. With more powerful apparatus the shocks are insufferably painful, when one hand is immersed in one portion of salt and water, and the little finger of the other hand immersed in the other portion, both being properly connected.

shock could be felt. When the iron bar was introduced, smart tinglings in the fingers. The spark was nearly alike in both cases.

Experiment 8.—With the large electro-magnetic wires (Experiment 2) in the circuit, no shock could be felt: the sparks and scintillations much brighter than with the other battery. The magnet in this case would lift more than 200 pounds.

Experiment 9.—When the electro-magnet was removed, and the coil of 300 feet placed in the circuit, smart shocks and bright sparks were obtained.

Experiment 10.—Another helix was now made of 110 feet of copper wire, similar to that in the former helices. This helix was nine inches long, formed on a pasteboard tube which would just admit a cylindric iron bar of one inch diameter: the cylinder was 12 inches long. A new zinc was made for the large battery (Experiment 1), and the liquid employed was cold salt and water. This helix, without the iron, gave no shock; with the iron, gentle tinglings only. The sparks were nearly alike in both cases.

Experiment 11.—The battery in this experiment was brought to great activity by nitrous acid and water, the same helix (Experiment 10) again forming the circuit. Without the iron no shocks could be obtained; with the iron in the helix slight shocks were perceptible. The sparks and scintillations nearly alike in both cases.

Experiment 12.—When the wires of the electro-magnet (Experiment 2) formed the circuit of this active battery, no shocks could be obtained. The sparks and scintillations were exceedingly fine. The magnet in this case would lift 400 pounds.

Experiment 13.—When the coil of 300 feet of wire (Experiment 3) formed the circuit, the shocks were very smart.

Remarks.

In all the hitherto described experiments, the coil of 300 feet of wire gave much the strongest shocks, although no iron was connected with it, proving in the most ample manner that the Magnetism of the iron employed is not the *sole* cause of the shocks.

That shocks should be produced by 60 feet of wire and none by 480 feet, although the latter was aided by a ferreous magnetic power, more than 300 times that of the former (compare Experiments 4 and 12), is a fact exceedingly curious, and one which could hardly have been predicted by those who have referred the principal operating power to the magnetic action of the enclosed iron; and that the coil, without any iron whatever, should produce stronger shocks than the coils with iron, is a fact still more at variance with those views.

It is obvious, however, by these experiments, that the Magnetism of the iron, under some circumstances, becomes efficient; and therefore the principal mystery rests in its

not being efficient in all cases, and how it should fail when produced in greatest abundance. Whilst contemplating these facts, it occurred to me that the cause of the superiority of the 300 feet coil over the other arrangements might probably be traced partly to the greater length of circuit, and partly to the fashion of the coil; and if so, similar coils with the same length of wire, the one *with* and the other *without* an iron nucleus, ought to show a difference of action. This, however, had already been done, under some circumstances, in my previous experiments, without being productive of much information on this point. There still, however, seemed a probability of the figure of the iron being concerned in the process, and especially if the action was that of Magnetic Electricity, as the results of this last series of experiments had partly indicated.

In Magnetic Electricity it is well known that the shocks principally depend upon the length of the coil wires—at least up to a certain extent; but the sparks and calorific effects are best developed by shorter and stouter wires, or, which amounts to the same thing, by shorter wires and more of them. This is in exact accordance with the facts developed by the experiments I have last detailed. The strongest shocks were obtained from the longest circuit, but the largest sparks and brightest scintillations from the greatest quantity of conducting matter in the circuit, in comparatively short lengths.

With these novel views, I entertained hopes of being enabled to modify the effects—from the iron by giving to it different forms, and from the wire by altering the fashion of the coils only, without any variation in the figure of the iron. Proceeding to experimental investigation, the first point I wished to determine was that of the influence of a straight bar in a long conducting wire. This could not be very satisfactorily ascertained, only by the employment of a battery of steady and uniform action during the period occupied by the experiments, a series of which were carried on in the following manner:—

Experiment 14.—A new zinc was now made for the smaller battery—the zinc amalgamated, and the exciting liquid dilute sulphuric acid. A wire 300 feet long, and similar in every respect to that coiled on the wooden bobbin (Experiment 3), was wound round about two inches and a half of the central part of a cylindric bar of iron, one inch in diameter and twelve inches long. This coil and that on the wooden bobbin were alternately, for twenty times, placed in the circuit of the battery. In every trial the coil round the iron gave the greater shock.

Experiment 15.—The question now to be decided was—can the action of the iron be made null when covered with one of these wires? Or can it be made to operate in a negative capacity by lessening the force which the coil alone would exhibit? To ascertain this point, a similar bar of iron to that used in the last experiment was bent in the middle and formed like a horse-shoe magnet, with its two branches as straight

and parallel to each other as they could conveniently be made. They were also brought pretty close to each other—all these particulars being considered to be essentially concerned in the action. This piece of iron was covered with the 300 feet of wire which had previously formed the coil on the wooden bobbin (Experiment 3). The wire formed six strata of coils, whose convolutions were close packed together. When this coil wire formed the circuit of the last-mentioned battery (Experiment 14), the electro-magnet would carry upwards of 80 pounds. It was now placed in the circuit alternately with that wound round the central part of the straight bar (Experiment 14), and shocks taken more than twenty times from each. In every trial the shocks were much the strongest from the coil on the straight bar—showing again that the shocks do not depend upon the *quantity* of Magnetism displayed, but upon a proper application of it.

Experiment 16.—The wire was taken off the straight iron bar, and wound in a close packed coil on the wooden bobbin. This done, the coil now formed was tried against that round the horse-shoe (Experiment 15), and found to be of superior efficacy in producing shocks.

Nothing could be more decisive than the results of these experiments in proving that that form of the iron most suitable for magnetic display is the least so in the production of shocks. They prove also that the iron may even be detrimental when used for the latter purpose.

Notwithstanding the satisfaction which I felt in my own mind respecting the lessened influence of the iron by bending it, there yet appeared one circumstance connected with the experiments, which, because of the possibility of its being a means of modifying the results, might porobably create doubts respecting the conclusions I have arrived at. It will have been observed that in making the experiments with the straight and bent iron bars (Experiment 15), the two coils of conducting wire which were formed on them were of very different fashions—that on the straight bar being a short thick coil, whilst that on the horse-shoe bar was much longer and thinner, covering the iron from one end to the other.

Experiment 17.—To prevent any misunderstanding arising from the above circumstance, I took the 300 feet of wire off the wooden bobbin, and coiled it round the straight iron cylinder in as nearly as possible the same manner as that coiled round the bent one, it being impracticable to make both helices precisely alike, because of the different shapes of the iron. This coil and that on the bent bar were alternately placed in the circuit of a battery of steady action for several successive times, and shocks taken from them individually. Those from the coil inclosing the straight bar were, in every trial, much stronger than those from the other coil. In this case the coils were of the same fashion, the wires forming them of the same length and thickness, and the pieces of iron of the same dimensions.

Experiment 18.—This experiment was intended to determine the only remaining point which appeared to be of much interest respecting the *modus operandi* in these curious phenomena, and the results show most decidedly that the fashion and position of the coil on the same piece of iron have considerable influence in modifying the phenomena.

The 300 feet of wire were taken off the bent iron bar (Experiment 15), and wound in a close-packed coil round the central part of a straight iron cylinder of precisely the same dimensions as the former (Experiment 14), and the coil of the same fashion as there described.

This coil, and that covering the other straight iron cylinder (Experiment 17), were alternately placed in the circuit of a battery of steady action for twenty successive times, and shocks taken from them individually every time. In every trial the shock from the short thick coil was much stronger than that from the long coil which covered nearly the whole length of its inclosed iron. This is a very interesting fact, as will appear obvious from a due consideration of some of the circumstances connected with it.

For instance, the iron inclosed in the long helix became a more powerful magnet than that round whose centre the short thick helix was formed. Moreover, the mean distance of the wire forming the *long* helix from the iron was much less than the mean distance of the wire forming the short helix from its iron, which, as far as the Magnetism of the iron is concerned, is another advantage.

From my first experiencing the shock from a coil, the effect appeared to me to be that of an electro-momentum; but, from the hurried manner in which my experiments were made and described, I had not sufficient time allowed to consider the nature of the action with that care which it evidently demanded. The more recent investigations which I have now detailed having furnished more varied theoretical data, I have been better enabled to perceive the connection of the phenomena and their causes; and to enhance the display by a proper application of the laws which govern the action. Still viewing the shocks as the effects of electro-momenta, I was again led to try double conducting wires, and have found that, under certain circumstances, the shock is more powerful than with one wire only. Three or four wires may be advantageously employed provided certain rules be attended to.

The length of the circuit most efficient in the production of shocks will depend upon the intensity of the battery; and the number of strands to be introduced to the circuit must be regulated to the extent of the battery's surface.

I have long ago entertained the idea that the first efforts of an electric current to force its way through a *coiled* conductor would meet with a greater degree of resistance than it would have to contend with in a straight wire of the same kind and dimensions, although the resistance to a current already in circulation would be less

in the coil than in the straight wire. I had barely time to allude to this supposed resistance whilst hastily writing the postcript to the preceding Memoir (see page 287), without meditating on any experiment for ascertaining the correctness or incorrectness of the idea I had formed of its existence. When I first read Dr. Faraday's *Ninth Series of Researches*, towards the latter end of October last, I was much interested in finding that similar views had been taken by that philosopher, although it did not appear that the experiments intended for demonstration were sufficiently conclusive to set this curious question at rest. The inquiry, therefore, being still incomplete, I was induced to undertake the following mode of experimental investigation.

As the contemplated resistance, whatever might be its amount, could only have a momentary existence, its detection appeared most likely to be accomplished by *transient** deflections of the Galvanometer needle. The Galvanometer employed consists of a rectangular coil of copper wire, each side of which is six inches long, and composed of eight convolutions, having a four inch needle with an agate cap, supported on a pivot, in the centre; consequently at the distance of three inches from the upper and lower sides of the coil. The battery is a small pair of cylinders of copper and amalgamated zinc, excited by very dilute sulphuric acid, so as to keep up a pretty uniform deflection of the needle of 20° with the *permanent* current through 300 feet of wire, which was the length of the circuit chosen for these experiments. Two wires of 300 feet each, and well covered with silk, were employed; one of which was formed into a compact coil on a wooden reel, having a hollow axis, and about two inches long. The other wire was hung upon chairs in loose folds which could have no action on each other.

These wires were used separately and alternately, for two experiments each, throughout the whole series; and, as the *initial* force of batteries in feeble action depends very much upon the interval of repose between any two completions of the circuit, improving every moment for the first minute at least, and perhaps for a much longer period, a standard interval of time for the battery's repose between every two experiments was strictly observed. The plates of the battery were not disturbed during the whole time. The interval of repose was one minute prior to each experiment. The period of electric action in each experiment was $2\frac{1}{2}''$, the needle having always attained its highest point of deflection within that time. The meridian line of the card was placed in the plane of the coil, from which position the needle started in each experiment.

The following table shows the results:—

* The distinctive indications of transient and permanent deflection are explained in the twentieth Memoir.

Tables of Transient Deflections with the Coiled and with the Uncoiled Wire, in the Circuits.

300 feet coiled.		300 feet uncoiled.	
Exp.	Deflec.	Exp.	Deflec.
1	59°	3	65°
2	60	4	70
5	65	7	69
6	65	8	70
9	65	11	69
10	65	12	70
13	60	15	66
14	62	16	67
17	60	19	67
18	60	20	67
Mean,	62·5	Mean,	68·5

By taking the forces as the sines of half the arcs of mean deflection, we have those forces as 1·08487 : 1, nearly; and if the resistances in the wires be reciprocally as the deflecting forces, then the resistance in the coil to that in the loose wire will be as 1·08487 : 1.

With other wires and other electric powers, the ratio of resistance would doubtless be found to be very different to that exhibited by these experiments. But although this is the only series of experiments I have yet made to determine this point, the care with which they were conducted leads me to believe that in all cases the resistance will be found greater in a coiled wire than in an uncoiled one of the same dimensions.

W. S.

London, March, 1837.

AN EXPERIMENTAL INVESTIGATION OF THE INFLUENCE OF ELECTRIC CURRENTS ON SOFT IRON, AS REGARDS THE THICKNESS OF METAL REQUISITE FOR THE FULL DISPLAY OF MAGNETIC ACTION—HOW FAR THIN PIECES OF IRON ARE AVAILABLE FOR PRACTICAL PURPOSES—AND ON THE FORMATION OF AN ORIGINAL ELECTRO-MAGNETIC COIL MACHINE, FOR MEDICAL PURPOSES.*

FIFTEENTH MEMOIR.

About some seventeen years ago a considerable degree of interest was excited in the philosophical world by some singular and unexpected facts which were discovered by Mr. Barlow, in his well-known series of experiments on the Magnetism exhibited by soft-iron when under the influence of terrestrial magnetic action only. The experiments of that justly-celebrated philosopher, to which I shall have more particularly to allude in this paper, are those by which the following curious fact was developed—viz. that the extent of magnetic action exhibited on a compass needle, by this class of ferreous bodies, depends principally on the *extent of surface* which they expose, and not on the mass or quantity of iron which they contain. Notwithstanding, however, the generality of this law as regards the influence of surface, it was very natural to imagine that some subsidiary law must necessarily be in operation with reference to the *thickness* of metal absolutely requisite for the full development of the magnetic action due to any given extent of surface. This obvious inference was not likely to escape the attention of the able philosopher who was conducting the experiments. Mr. Barlow accordingly furnished himself with the necessary specimens, both as regards *thickness* and *quality* of iron, for the complete investigation of this philosophical problem, the solution of which, whether regarded in a theoretical or practical point of view, appeared to be of the highest importance to the science of Magnetism.

Mr. Barlow's experiments appear to have been conducted with great care, and through an extensive series of ferruginous specimens; and from the results which they afforded, it appeared that iron of about *one-tenth* of an inch in thickness† displays

* Read before the Electrical Society of London, August 5, 1837.

† By referring to the table of "Experiments of Plates of Iron," it will be found that *chest iron* 0·1384 of an inch thick gives the greatest deflection. Mr. Barlow concludes, however, that the necessary thickness is "probably one-twentieth of an inch."

as much magnetic action on a compass needle as though it were of much greater substance ; and another series of experiments, subsequently conducted by Captain Kater, was attended with similar results.

This important law, which governs what may conveniently be considered the *natural* display of the Magnetism of soft iron, reduces the standard action of immense masses much below that which would have been exhibited by them, had it been proportional to their solid contents; and which would probably have amounted to an almost unmanageable extent on the steering compass, when emanating from the great number of heavy pieces of iron which now enter into the construction and equipment of large men of war; and, on the other hand, this law places at our command an immense magnetic action from a comparatively small quantity of iron, by a mere extension of its surface into any required form.

By taking advantage of this law, in combination with that which governs the action by *proximity*, Mr. Barlow was enabled to counteract the magnetic effects of all the iron on board of our largest ships of war, by the employment of a thin disc of that metal, not more than twelve inches in diameter.

Notwithstanding the importance of the investigations I have now mentioned, nothing has hitherto appeared of a similar character, as regards the action of soft iron when under other influences than those of terrestrial Magnetism. About some four years subsequent to the celebrated Œrsted having laid the foundation of Electro-Magnetism, I had the good fortune to discover that bars of soft iron, subjected to the influence of electric currents, display magnetic action in a very eminent degree, and to an extent far beyond that which iron usually exhibits by any other mode of excitation. I also showed that the magnetic force, thus brought into play, may be annilated—re-produced—and its polar character reversed with any velocity which the experimenter has at command for the requisite transition of the electric currents employed. These facts have been amply corroborated by subsequent experiments ; and latterly their importance has become considerably enhanced by their being *those alone* on which the prevailing hope now rests of bringing the astonishing agency of Electro-Magnetism into practice as a first mover of machinery, and as a motive force of general application. It is in the latter capacity—especially in the application of this force to purposes of locomotion—that the determination of the requisite thickness of soft iron for the full development of its Magnetism, when subjected to the influence of electric currents, becomes a consideration of the first importance, in order that the vehicle in which it is employed may be entirely free from an incumbrance not essential to the production of the force which the iron is susceptible of displaying in the shape best adapted to the construction of an engine; and which, whilst solid masses are employed in the construction of large engines, may possibly amount to an extent sufficient to neutralize a considerable portion of the power absolutely produced, and

perhaps to completely extinguish the most ardent hopes, and frustrate every design of those who enter on this laudable pursuit.

It is now about nine years since my attention was first directed to this important branch of electro-magnetics, and shortly afterwards I instituted a series of experiments for a full investigation of the subject. The results of these experiments have never yet been published; and as nothing of the kind has hitherto appeared from any other quarter, they are yet as new to the scientific world as of yesterday's discovery, and I hope of a sufficiently interesting character to offer to the notice of the Electrical Society.

The first point necessary in this inquiry was simply this—do *hollow* pieces of iron display magnetic action to the same extent as *solid* pieces of the same figure and dimensions, when both are submitted to the influence of similar electric currents? To decide this point, and others connected with the investigation, the following experiments were instituted.

A piece of musket barrel, about twelve inches long and about nine-tenths diameter, was enclosed in a spiral copper conducting wire, No. 15, which was well covered with sewing silk. The convolutions of the spiral were placed as close to each other as the insulating silk would permit, and the spiral covered nearly the whole of the iron tube from one end to the other. A solid cylindrical bar of soft iron of a little smaller diameter than the bore of tube, was also provided, for the purpose of being introduced to, or withdrawn from, the interior of the latter at pleasure. The tube was also moveable in the coil (which was formed on a paste-board lining), and could be taken out and replaced by other pieces of iron, whose effects might be found necessary to ascertain in the investigation. A magnetic needle having an agate cap, and supported on a fine steel point, placed in the centre of a graduated card, was also provided.

The axis of the spiral was placed at right angles to, and in the same horizontal plane with, the magnetic needle, having its nearest extremity opposite to the centre of that instrument, and twelve inches distant from its pivot. The source of electric action was a single Voltaic pair of copper and zinc, placed in a porcelain pint jar: he exciting liquid diluted nitrous acid.

When an electric current from this battery traversed the helix, the needle deviated to 3°. The iron tube was now placed in the spiral, and the deviation of the needle increased to 30°. This ascertained, the solid iron cylinder was slowly introduced to the bore of the tube, and the deflections of the needle noted at every inch which the former advanced towards the needle in the interior of the latter. The results are exhibited by the following table. When the solid bar was made to touch the remotest end of the tube the deflection was 40°.

When introduced.	Deflections.	When introduced.	Deflections.
1 Inch	45°	7 Inches	40°
2 Inches	46	8 "	37
3 "	47	9 "	35
4 "	45	10 "	33
5 "	44	11 "	32
6 "	43	12 "	30

These experiments show that the deflection increases for the first three inches of the solid bar's introduction, but no further; and that it again diminishes for every advance of the bar from that point, and when the whole is enveloped by the tube, the deflection is about the same as by the latter alone.*

There was something so exceedingly curious in the results of these experiments, especially in the loss of magnetic action as the solid bar advanced towards the needle, that I was induced to repeat them many times, but they were always attended with similar results. I next examined the magnetic action of the external extremity of the solid cylinder when introduced to various distances in the tube, but nothing very interesting was discovered, its polarity being of the opposite kind to that exhibited by the end of the tube nearest to the needle, as was expected.

Other pieces of iron of various shapes and magnitudes were made to touch the furthest end of the tube, whilst under the electro-magnetic influence, and in every case a considerable increase of deflection was observed; but in none was the deflection so great as when the solid cylinder was introduced to about two or three inches into the bore of the tube. I have sometimes observed about two degrees greater deflection when the whole of the solid cylinder was introduced than when not present; but this is not always the case, and therefore does not interfere with the general results.

The next experiments were made with another piece of the same musket barrel, but, being nearer the muzzle of the piece than the former, was of somewhat thinner metal. The thinner end of this tube was closed with a solid plug of iron, firmly welded to it, and reached about half an inch inside.

When this tube was placed in the electro-magnetic spiral, with its solid end nearest to the needle, the deflection was about 2° less than with the former open tube. Several trials were made by changing them frequently in the spiral whilst the same current was traversing it, and the mean difference about 2°. The increase of deflection, by introducing the solid iron cylinders, was similar to that shown by the former

* This experiment was shown to Mr. Christie, in the summer of 1830, in the Royal Arsenal, at the time that I was carrying on my experiments on the Thermo-Magnetism of simple metals, and magnetizing some of the largest pieces of iron ordnance, and iron globes, both solid and hollow, by the influence of electric currents. A ten inch bomb sheel, which I fitted up for the lecture table, has been exhibited in the Adelaide Gallery of Practical Science for the last five years, and remains a prominent piece of electro-magnetic apparatus of that excellent institution.

open tube; with this exception, when the solid cylinder was introduced as far as possible, or till it touched the inner end of the plug, the deflection was invariably about 3° greater than when the solid cylinder was not present. This effect I attributed to the thinness of the metal at that end of the tube, which was of very different dimensions on the opposite sides. On one side of the tube the iron was one-twelfth, and on the other not more than one-twentieth of an inch thick.

Another iron tube, of the same external dimensions as the former, but not more than one-thirtieth of an inch thick of metal, was next placed in the helix: the needle deviated to 22°. When the solid iron cylinder was wholly introduced to the interior of this tube, the deviation increased to 32°; and the needle stood deflected to 30° even when the solid cylinder was withdrawn. This curious circumstance induced me to vary the experiment in several ways. I heated the tube to redness and destroyed every trace of local magnetic action, so that when held vertical, the *lower* extremity, whichever it might be, invariably exhibited the same kind of polarity. This treatment of the tube, however, though it lessened the quantity gained and retained, did not entirely remove the latter effect. The tube still exhibited greater magnetic action after the solid cylinder was withdrawn than before it was introduced. Tapping the tube with a piece of wood, so as to agitate its particles whilst under the influence of the current, increased its magnetic action so as to deflect the needle two or three degrees more than before such treatment; but by no method which I could think of would it become so powerful as by the introduction and removal of the solid cylinder; and the effect was nearly the same whether the current was traversing the helix at the time the solid cylinder was introduced, or that the connections were made afterwards. The tube, with its solid cylinder, was introduced into the interior of the helix at one and the same time, both whilst the current was moving through it and before the battery connections were made; but still the effects were of the same kind, and nearly to the same amount in all cases. It occurred to me eventually, that since the deflection was greatest whilst the whole of the solid cylinder was in the tube, the former had become as decidedly polar as the latter, and would consequently retain *some* polarity whilst any part of it was within the helix. Under these circumstances the operation of the interior pole of the solid cylinder, whilst being withdrawn from the tube, would be on the latter, similar to the operation of any other magnet upon a ferruginous body, whilst rubbed over its surface. It would indeed magnetize the tube, which now being subjected to an auxiliary influence conspiring with that of the current, would become more active than by the influence of the latter alone. This explanation seemed to be satisfactory enough as far as regarded the magnetizing of the tube, but was no reason for the latter retaining nearly the whole of the additional power exhibited whilst the solid cylinder was wholly in the interior. The mystery, however, seemed to be reduced to the following question.

Are electric currents, which alone magnetize a piece of soft iron only to a certain degree of power, capable of retaining any portion of an additional power conferred on the iron from any other momentary source of excitation?

To set this question at rest, the tube alone was placed in the helix, and the needle deviated to 25°. The proper pole of a steel magnet was now made to touch the furthest end of the tube, and the needle deflected to 70°. When the permanent magnet was withdrawn, the angle of deflection subsided to 35°; so that the tube, by this treatment, had acquired an additional power capable of pulling the needle from 25° to 35°, which it retained for ten minutes, the time of electric action after the magnet had been taken away. On disuniting the battery and helix, without removing any part of the apparatus, the needle, after a few oscillations, reposed in the meridian—proving that, although the iron tube might still retain some trace of permanent polarity, it was incapable of affecting the needle at the distance of twelve inches; and consequently the action left was infinitely small when compared to that which was retained by the electric current.

The appearance of this novel fact induced me to place the pieces of gun barrel in the helix again, and subject them to the same treatment as the thin iron tube had been placed under. Both pieces retained a considerable quantity of the additional power conferred on them by the permanent magnet, whilst they remained under the influence of the current, but lost all trace of it when the current was cut off.

A solid cylinder of iron of the same dimensions as the hollow tubes was next placed in the helix: the needle deviated to 28°. The permanent magnet was now brought to the furthest extremity of the iron, and the deviation increased to about 74°. When the magnet was withdrawn, the angle of deflection became reduced to 32°. When the electric current was cut off, the needle soon reposed in the magnetic meridian.

Returning to the principal subject of investigation, I next compared the electro-magnetic action on the tubes and solid cylinder of the same dimensions: the two pieces of gun barrel invariably gave greater deflections of the needle than those shown by the action of the solid piece of iron, but the thin tube gave smaller deflections.

Cylindrical rolls of sheet tin (tinned iron) gave greater deflections than the thin iron tube, but something less than either piece of the musket barrel. A bundle of iron wire gave smaller angles of deflection than the thin tube, and a cylinder of copper gave no motion to the needle.

From the phenomena developed by the preceding series of experiments, it appears that iron tubes of a certain thickness of metal, perhaps about one-tenth of an inch, are susceptible of displaying as much magnetic force as solid pieces of the same

dimensions, when both are submitted to the influence of a *single** electric current, proceeding from a single Voltaic pair of copper and zinc. This important fact is analogous to that discovered by Mr. Barlow, and now places at our command an immense magnetic force with a comparatively small mass of iron—a force which may be increased to any required extent, and by a process the most simple that could be imagined. The principal encumbrance now removed, what can be the remaining impediment to the full completion of electro-magnetic engines for any purposes they may be wanted? The old engines of solid iron have worked well; the new ones, with hollow iron, will work better.

We also discover, by these experiments, that electric currents are capable of retaining in active play a greater degree of magnetic action than that which themselves alone are capable of exciting. Hence electric currents exercise two kinds of magnetic power on soft iron—the *exciting* power and the *retentive* power. The difference of these two powers may very conveniently be called the *adscitious* power or force, because it is that which the current displays in addition to the *exciting* force; or that which is required above the latter to complete its maximum of magnetic action.

With the exception of a few electro-magnets which I constructed for rotations, the experiments I have hitherto described were the only ones I made on hollow pieces of soft iron, until the account of Professor Henry's experiments with the spiral conductor reached this country. Since that time, however, it is well known to the scientific world that I have been much engaged in investigations with the coil-conductor; and have varied its form, and also the fashion of the axle-iron, in as many ways, perhaps, as any one pursuing the subject. The results of some of those investigations are already well known, but there are others in reserve which I may now be permitted to describe.

The helix which I now employ consists of two distinct wires, the one rather thick bell wire, the other very thin. The former is about 260 feet long, and forms the inner or lower coil. The thin wire forms the upper or outer coil, and is 1,300 feet long.† Both wires are covered with sealing-wax varnish, but no thread of any description. This covering has the advantage of permitting the convolutions to lie much closer together than when the wire is covered with thread, and the action becomes much increased in consequence. The reel or bobbin is of wood, two inches long inside the cheeks. The axis is hollow, for the reception of a bar or other piece of iron.

The coiling of the wire is proceeded with in the following manner:—One end of the thick wire passes from inside to outside of one cheek of the bobbin, at the bottom

* By a *single* electric current I mean a current transmitted by a single helix; for it is possible that when the current is multiplied by traversing more than one helix over the iron, that some other law may regulate the phenomena. This point I mean to investigate shortly.

† It is well known to the readers of the *Annals of Electricity* that Professor Callan employs two wires, similar to those here described; but the coils used by that philosopher are very different to those I am now describing. Professor Callan employs *long* coils, on iron horse-shoe magnets. Those which I employ are short and thick, with hollow axis.

of the bed. This end of the wire may be left of any required length for battery connection. The wire is now coiled in close convolutions until it arrives at other end of the bobbin. A strip of silk is now laid over the coil for insulation, and to prevent the wax being rubbed off by the next about to be placed above it. The coiling now proceeds back again to the first end of the bobbin. Now another strip of silk covers the second coil, and a third coil over that, and so on till the whole of the thick wire is nearly taken up, leaving only a few inches, which passes through one cheek of the bobbin, for connection with the other end of the battery. Through this wire, and this alone, does the battery or primitive current run; and it is from this coil alone that the spark is shown.

One end of the thin wire is now soldered to the last convolution of the thick one, and a strip of silk laid over the last coil; this done, the coiling of the thin wire is performed in precisely the same manner as the thick one: when the whole is put on the bobbin, the coil part of the apparatus is complete. The process is exceedingly tedious.

When the shock is taken from this helix, one hand is connected with the outermost end of the thin wire, and the other hand with either of the ends of the thick one. If the other hand is connected with the lower end of the thick wire, the secondary current has to traverse that wire. When the hands are connected with the two ends of the thin wire, the secondary producing the shock, runs through that wire only, and the effect is greater than by the other connections.

The helix now described is fixed to the base-board, A A, of the instrument represented by Fig. 9, Plate XII. One end of the *thick* coil wire is united by solder to the copper wire, *z*, and the other end of the same wire to the copper wire, *c*. The brass studs, *d* and *e*, with their balls, are united to the two ends of the thin coil wire. A pillar, P, fixed to the base-board, rises behind the helix and supports the wheel, *w*, and the axis, *a*, with its pulley, which is behind the pillar. The battery connections, with this instrument, are made by the copper wires, *z* and *c*. The current flows from the battery through the wire, *c*, to the lower extremity of the thick coil wire; and after traversing the whole length of that wire arrives, by proper connections, at the amalgamated copper disc, *m*, supported on a pillar, as seen in the figure. From the mercury in the disc, *m*, the current proceeds along the bent wire, *t t*, to the brass stud, *s*, which is fastened to the upright, P, and thence by a conductor to the wire, *z*, and thence to the zinc side of the battery.

One end of the bent wire, *t t*, is furnished with a socket which fits the vertical part of the stud, s, pretty tight: the other end is finely pointed and amalgamated, and dips into the mercury at *m*, at which place the circuit can be opened and shut with great rapidity, by means of a lifting piece or cam, which is placed on the axis, *a*, and which is made to revolve by the wheel and band, as seen in the figure. The revolving cam lifts the spring, *t t*, twice each revolution of the axis, *a*.

The wheel, *w w*, is six inches diameter, and the pulley on the axis, *a*, is one inch diameter; these being united by a band, causes the latter to revolve six times whilst the former revolves once: therefore, by turning the wheel at the rate of three revolutions in a second, the circuit is opened 36 times, and consequently 36 sparks are produced in that period. Instead of the copper disc, *m*, I sometimes fix a small bottle, containing a portion of mercury in its place, as delineated by the dotted outline at *m*. By this means, there is no scattering of the mercury, and the spark is seen in the bottle, whose reflection increases the light.

Another mode of opening and shutting the circuit rapidly is by means of a notched zinc disc, D, which, when the lifting piece is removed from the axis, *a*, and the wire, *t t*, from its stud, *s*, is fixed vertically to the pillar, *p*, and concentric with the axle, *a*. On the latter is fixed a spring wire, whose furthest extremity presses against the notched part of the disc. This wire revolves with the axle, and the point which touches the zinc disc passes over the notches, and consequently the battery connection is broken every time the revolving trigger arrives at a notch in the disc. The disc has 30 notches, which, multiplied by 6, the number of times the trigger revolves faster than the wheel, produces $30 \times 6 = 180$ interruptions of the circuit, and consequently as many secondary currents (with their shocks, if required) for each revolution of the wheel. The wheel can be made to revolve with ease three times in a second: hence $180 \times 3 = 540$ shocks can be communicated in one second; or $540 \times 60 = 32{,}400$ in one minute. When the room is darkened, a circle of sparks appears on the face of the disc.

When shocks are to be produced—if to the hands—the cylinders, *r r*, which are connected with the studs, *d e*, by means of wires, are to be grasped one by each hand, and the wheel put in motion when either of the discharging parts is attached to the apparatus. When shocks are to be communicated to any other part of the body, the common medical directors with glass, or other insulating handles, must be used—the metallic stems of the directors to be connected with the studs, *d e*, by thin wire, in the same manner as to a common electrical machine. The balls must communicate, either directly or indirectly with the skin, by similar means as those resorted to when Galvanism is applied.

The shocks are pretty smart, and the sparks tolerably bright from this instrument; but they are still more so when a cylindric bar of iron is placed in the axis of the coil, and the wheel turned at a moderate speed. If, however, the wheel, be turned rapidly, especially when the notched disc with its spring trigger is employed, the powers of the instrument, instead of being augmented by the iron axis, are absolutely diminished; and when the speed is very great, both sparks and shocks entirely disappear. When I first discovered this singular fact, I was led to suppose that it arose from an imperfect contact of the disc and trigger when the latter rotated with great rapidity; but

the noise that was made by the one scraping over the other convinced me that the cause was not a want of contact.

I now got an assistant to turn the wheel, whilst I slided the bar to and fro in the axis of the coil, sometimes taking it wholly out and again replacing it in the bobbin, the room being quite darkened. The brilliancy of the sparks underwent no change by any motion which I could give to the iron bar whilst in the interior of the bobbin. Nor could they be made to re-appear when the velocity of the wheel was great, so long as the bar remained within the bobbin; but when it was taken entirely out, the sparks invariably made their appearance.

I was now led to try the effect of my old servants, the iron tubes, in the interior of the bobbin; and I soon found that the shocks given, when a piece of the musket barrel was placed in the axis, were much stronger than any I had before experienced from the instrument. The other piece of gun barrel was tried, and the shocks were stronger than with the solid bar; and they were still more so by employing the thin iron tube before mentioned. The sparks were also brighter than with the solid bar, and appeared with greater velocities of the wheel; but in all cases they disappeared when the velocity was very great.

I next substituted a cylindrical roll of sheet tin (sheet iron tinned) for the iron bar: the shocks were now increased to an astonishing degree, and, with great velocities of the wheel, the sparks were much brighter than with any of the preceding pieces of iron in the axis of the bobbin. With a bundle of thin iron wire* in the axis of the bobbin, they were quite as powerful, if not more so, than with the roll of tinned sheet iron.

I placed a bundle of iron wires in the axis of the roll of tinned iron, but could not discover any additional effect. I also rolled in a compact coil a whole sheet of the thinnest tin I could procure, which was double the extent of surface of the roll previously employed; but this, when placed in the axis of the helix, did not produce such strong shocks as the thicker tin plate. Bundles of narrow strips of tin plate gave very strong shocks.

Some of the phenomena developed by this series of experiments are obviously analagous to some of those developed by the former. The scroll of thin tin plate, although double the extent of surface to the thicker, did not produce such powerful shocks as the latter, showing that a certain thickness of the iron is necessary for the development of a maximum effect; and I think it is very possible, though I have not had

* It is something remarkable that Mr. Bachoffner, who, about a fortnight before this paper was read, had purchased one of my coils, but without knowing any thing of my experiments, accidentally discovered that a bundle of iron wires in the axis of the helix caused it to give a better shock than when a solid bar was employed. When Mr. Bachoffner became acquainted with what I had done, he very politely expressed a wish not to publish an account of his experiments, which he had drawn up for the forthcoming number of the *Annals of Electricity*. To this, however, I could not think of consenting, knowing that his discovery must have been made independently of any knowledge of my investigations.

time to try, that a still thicker plate of tinned or any sheet iron might tend to increase the power of the instrument to some greater extent than any it has yet shown.

To understand how it happens that the iron increases the power of the instrument when the discharges are slow, but decreases it when they are made rapidly, requires two distinct investigations. The explanation of the former effect will be found in the principles of Magnetic Electricity already explained in this volume; but to explain the latter effect it will be necessary to call to our aid another principle in magnetics, which hitherto I have not named.

In all cases where a ferruginous body is inclosed in an electro-magnetic spiral, the magnetic lines of that body will be arranged in the opposite direction to the electro-magnetic lines of the inner surface of the spiral, which keeps them in play; and consequently in the *same* direction as the outer magnetic lines of the spiral, as may be understood by looking at Fig. 15, Plate XI. Now the phenomena exhibited by the machine being produced only at the time of the primitive circuit being opened, they are those of the *terminal* secondary current, and are the effects of a *collapsion* of both the electro-magnetic lines, and of the magnetic lines belonging to the ferruginous body; which, by operating in concert, give a series of exciting impressions greater than either of them would do alone. The large curved line in Fig. 15 is intended to represent the situation of those magnetic lines belonging to the iron which have distended to beyond the spires of the helix, and give exciting impressions by their collapsing motions.

To understand the cause of the *lessening* of the power of the helix, by opening and shutting the electric circuit with great rapidity, it will be necessary that we call to remembrance a well known fact which is observed whilst magnetizing a piece of iron, by the influence of electric currents. *Time* is required to produce a maximum of effect. And again, when the current is cut off, *time* is required for the iron to recover its neutrality. From the appearance of these circumstances, we are led to suppose that either the iron is a bad conductor of the magnetic matter, and impedes its motions, or that the latter, like all ponderable matter, is naturally and sensibly *inert*. Either principle alone would satisfy the conditions of the phenomena which I have named, and which have been long known, but it does not appear so obvious that the *novel* phenomena which I have described, can be owing to an inferior conductibility of the iron, independently of the operation of the other principle. I am led to suppose that there is a *magnetic inertia*, and that the magnetic matter is as prone to remain in its *last placed* condition as any other species of matter whatever.

The disappearance of the sparks and shocks by the introduction of the iron to the axis of the helix, could not possibly arise from an absolute torpitude of the magnetic matter belonging to the bar; for in that case it would be perfectly neutral, and the phenomena would be displayed with the same precision as if no iron were present.

That some peculiar counteracting force is in operation during the presence of the iron is sufficiently obvious; and as we have no knowledge of any other than the magnetic by which counter currents could be excited in the coil, it is allowable to infer that, in consequence of the *magnetic inertia*, in conjunction with the imperfect conductibility of the iron for Magnetism, the polarizations and depolarizations of the bar are *not simultaneous* with the polarization and depolarizations of the helix, but are invariably later: and when the *distentions* and *collapsions* of the electro-magnetic lines of the helix succeed one another in *rapid* alternations, the *opposite* motions, or the *collapsions* and *distention* of the ferreous magnetic lines, are respectively taking place. Both systems of lines are in motion, though similar poles meet one another. Both systems give exciting impressions, but in directions to produce opposite electric currents. A feeble current will result from the excess of the one over the other. When the circuit is opened and shut very rapidly, all the phenomena of secondary currents entirely disappear, even when no iron is present—a fact not easily accounted for upon any other principle than either *magnetic* or *electric inertia*, or upon both.

I cannot close this Memoir without pointing out the advantages that may be derived by employing a helix such as I have described, with any contrivance for producing a rapid succession of shocks, as a medical electric apparatus. A cylinder of copper and zinc, which would enter a pint jar, if excited by salt and water only, will be a sufficient battery for the instrument to produce very smart shocks. The expense of keeping up the power is thus reduced to a mere insignificancy, and the first cost is trifling. A strong shock and bright spark are produced by this instrument, when the battery employed consists of a copper and zinc wire, No. 15, immersed one-tenth of an inch deep into dilute nitrous acid placed in a watch-glass.

W. S.

London, July, 1837.

P.S.—Shortly after this Memoir was read before the Electrical Society, the instrument described was seen by several scientific gentlemen, both in London, Manchester, Preston and Liverpool, who have considered it as the most portable, efficient and economical electrical machine ever yet offered to the notice of the medical practitioner; and it has now, 1849, attained a greater celebrity than any other electro-medical apparatus whatever.

ON THE APPLICATION OF THE THEORY OF MAGNETIC-ELECTRICITY, IN EXPLANATION OF THE PHENOMENA EXHIBITED BY ELECTRO-MAGNETIC COIL MACHINES, TO SECONDARY ELECTRIC CURRENTS; AND ALSO TO CURRENTS OF THE THIRD FOURTH, &c. ORDERS.

SIXTEENTH MEMOIR.

The singular influence which an electric current exercises in bringing into momentary activity the dormant electric energies of an adjacent wire, requires considerable attentiveness and much thought to comprehend the manner of its action. Its contemplation requires a previous well-grounded knowledge of the proximate laws which govern the reciprocal excitation of Electrics and Magnetics; and the most profound ideas respecting the operation of these laws in the invisible processes by which those powers are productive of each other's phenomena.

Induction, influence, reaction, and other fashionable conveniencies, although satisfactory enough to express generally that *some* force is in operation, give no intelligence whatever respecting the nature of that force, and consequently indicate no mode of its action, To *guess* that this or that power is in operation, may possibly stimulate to inquiry, and occasionally become useful in that humble capacity; but conjecture without principle implies imperfect knowledge, and can never be regarded as the offspring of sound philosophical reasoning.

Facts may be produced and phenomena predicted by those who are in possession of certain rules which have become established from observation; but the primary laws from which these rules and those phenomena emanate are existences of a very different order. The perfect invisibility of the process by which these laws operate precludes its cognition by the external senses, and renders it comprehensible to the mind only. All our reasoning, however, on the invisible operations of nature must necessarily be based on those which, by their conspicuousness, have become perfectly familiar to us, so that by applying the one to the phenomena of the other we may be enabled to ascertain whether or not the same law be applicable in both cases.

That the laws which govern the production of secondary electric currents, by the influence of primary ones, are still obscured in mystery may be justly inferred from the fact, that no attempt has hitherto appeared to throw the least gleam of light on their development. The principles of action may possibly have appeared too recondite for

development, but can never be considered as too uninteresting to deserve attention. The whole theory of Electro-Magnetism and Magnetic Electricity hangs upon them.

Why, it may now be asked, do *secondary* currents run counter to *primitive* currents by the first impulses of the latter, and in the same direction as the primitive by the last impulses? Why also are the energies of the first-named secondaries much feebler than those of the latter secondaries? And why do secondaries almost annihilate the terminal effects of primitives? These facts, which are developed either with or without iron, have not hitherto been referred to any definite cause, nor indeed has any attempt yet been made at explanation. They are, it is true, amongst the most mysterious phenomena presented by this branch of physics, and the laws by which they are exhibited the most difficult of access, and interwoven with curious and intricate complexity; but there are others of less difficult explanation, whose sources of action are still permitted to remain in concealment. The equality of *action* and *reaction*, a law so generally admitted into reasonings on physics, although not refuted by these curious phenomena, afford no assistance in the solution of the mysterious problems which they present. The renitency encountered in the conductors will necessarily exercise a due influence in lessening the force of secondary currents, but cannot be made available as a cause of the comparative atony which these currents, by the initial impulses of the primary, invariable display.

The laws which govern these interesting phenomena do not, however, appear to be too deeply hidden for cognition; and although, in some instances, a complexity of action is discoverable, which tends to conceal the operating forces, they do not appear to me to be entirely precluded from access, nor insusceptible of explication. Electricity and Magnetism are here, however, playing their nimble powers on each other in the most profound retirement; their motions, concealed from corporeal vision, permit of no other approach than by the perceptions of the mind, and, by that mind only, already perfectly familiar with the proximate laws of Magnetic Electricity.

These laws, as they have appeared to me, are clearly and expressly enunciated in the eleventh Memoir; and if their present application to the generation of electric currents, whose developments are shrouded by the most complex circumstances hitherto known, should appear legitimate and conclusive, a more severe test can hardly be wanting to establish their universality in this branch of physical science.

1.—The most simple application of this theory is in the production of electric currents by the motions of conducting bodies in the vicinity of a bar-magnet. Let o, Fig. 10, Plate XI. represent a transverse section of an endless metallic wire, situated in the magnetic atmosphere of the bar, N S, whose *polar lines* are consequently some on one side and some on the other of that portion of the wire represented by the section, o. If now the wire, o, be made either to approach the bar or to recede from it,

it will have to pass through some of those exciting magnetic lines. Hence, by position 2, the electric fluid in the wire will be put into motion.

If the wire be made to *approach* the steel bar, it will then advance on those magnetic lines situated between them; and, according to positions 6 and 7, the direction of its electric current will be *from* the spectator, looking at the figure, to behind the paper on which it is printed, and where the wire is supposed to be continued; but if the wire be made to *recede* from the magnetic bar, its electric fluid will be excited by the impressions of those polar magnetic lines which are *exterior* to it, and the current will flow *towards* the spectator, or in the opposite direction to the former current. It is obvious, however, that although the currents thus produced will flow in directions the reverse of each other in the wire, and also with regard to the position of the magnetic bar, they still observe *one* and the *same* direction with reference to those magnetic lines which impel the electric fluid into motion. Precisely the same phenomena would be displayed if the wire were stationary, and the magnet put into motion.

Remark.—By inspection of Fig. 10, it will appear obvious that the currents will observe these directions in all cases where the advance and recession of the wire are between the extremities of the bar, and in a plane perpendicular to its axis.

2.—If the wire be kept perpendicular to the axis of the magnet, and passed down the side of the latter from the upper extremity, N, to its centre, the efficient magnetic lines will have the same relation to the wire as those to the *advancing* wire in the first application, and the current will be *from* the spectator; but if the wire be continued in its downward motion farther than the centre of the magnet, it will then advance on the efficient magnetic lines in the opposite direction to that whilst moving down the first half,* and the current thus produced will be *towards* the spectator. By moving the wire in the opposite direction, or from the lower to the upper extremity of the magnet; then, because of its advancing upon the magnetic lines, in precisely the same order as before, whilst moving downwards from N to S, the currents thus produced will observe the *same* directions whilst the wire passes the first and second halves of the magnet respectively; so that the current produced opposite the lower half will be *from* the spectator—that produced opposite the upper half will be *towards* the spectator. Hence this practical

Rule.—If a wire be placed at the centre of the magnet, and at right angles to its axis, then if it be moved parallel to itself, either towards the north or the south pole, the electric current produced in that wire will always observe *one* and the *same* direction; but if the wire be moved from either pole, *towards* the centre of the magnet,

* This fact would appear more obvious if the curve lines opposite each half of the magnet were to be resolved into two systems of right lines, one *parallel* and the other *perpendicular* to the axis of the magnet, as represented by Fig. 17, Plate XI—the latter systems only, in this case, being those engaged in exciting the electric currents.

the current produced will be in the *opposite* direction to the former—the *directions* of both currents being conformable to the law laid down in Positions 6 and 7 of the theory; and as the same arrangement of *magnetic lines* is observed on every side of a magnet, the *rule* holds good in the motions of rings, or endless flat helices placed on the axial bar; or, when those forms of conductors are stationary, by the motions of a magnet in their axis. In these cases the exciting impressions take place on every side of the ring or helix.

The same laws of excitation apply to the phenomena exhibited by the employment of a horse-shoe magnet as to those exhibited by that of a straight bar, and may easily be understood by an attention to what has been said respecting the latter kind. If, for instance, one portion of an endless wire were to be placed between the branches of the horse-shoe magnet, represented by Fig. 4, and perpendicular to its plane, then a motion of that part of the wire, parallel to itself, from the bend of the magnet towards its poles, would advance it nearly perpendicularly on those magnetic lines situated immediately between the branches; and also on those curved magnetic lines which are above and below the space between them, and whose poles are in the same direction, as may be understood by looking at Fig. 5. Hence, by *Positions* 6 and 7, the current produced in that part of the wire would be *from* the spectator, looking at Fig. 4; but if the wire were to be moved in the opposite direction, or from the poles towards the bend of the magnet, the current in the same part of the wire would be *towards* the spectator, both currents with this magnet observing the same laws as with the straight one, and invariably referrible to the polar positions of those magnetic lines on which the wire advances.

Imagine now the endless wire to be a ring which will move freely on either branch of the magnet, and its motions similar to those before described. Then, although the directions of the currents and motions of the ring will still observe the same relation to each other as before, each current, excepting in that part of the ring immediately between the branches of the magnet, will *appear* to change its direction, by the motions being made on this or that particular branch, as may be understood by looking at Fig. 11, where the arrows indicate the directions of *one* and the *same* current in the ring, when placed on different branches of the magnet. The imagination may possibly be assisted by considering the original ring to be split, or composed of two flat rims laid side by side, and susceptible of separation, excepting on one side, where they would still be held together. These two portions of the original ring being now placed on the poles of the magnet—one on each, as in Fig. 11—and moved *from* the poles towards the bend, would each carry a *branch current* from that excited in the *whole* part of the original ring situated more immediately between the magnetic poles. Behind the magnet, one of these branch currents would flow towards the spectator's right and the other towards his left, whilst the *main current* between the branches of

the magnet would flow directly *from* him. Reversing the motion of the wire would be attended with the usual vicissitudes in the direction of the current.

The *apparent* anomaly exhibited by these phenomena, which is a mere deception, arising from the figure of the magnet requiring those parts of the ring not immediately between its branches to be placed towards the right of the spectator in one case, and towards his left in the other, has been productive of much mystery, wherever a perfect knowledge of the fundamental laws has been wanting. The same illusion is effected by operating with the poles of a bar magnet, provided they be placed in one and the same direction (say upwards) during the time they are employed.

All that has been said about rings apply to helices of every description.

By discovering a similar illusion in M. Ampere's beautiful experiment, in which a magnet rotates on its axis, I was enabled to rotate a large magnetic bar, by causing two electric currents to traverse it at the same time, each half its length, from its poles to its centre, or conversely; which currents, according to the views previously taken of the nature of the action, ought to have counteracted each other's effect. (Third Memoir, page 96.)

4. Let the bar, N S, Fig. 10, be a cylinder of soft iron, and O the section of a wire placed at right angles to it. If now the iron bar be converted into a magnet similar to that represented by the figure, by the application of the poles of permanent magnets at its extremities, polar magnetic lines will start from its surface on every side; and those on the right side will advance upon the wire, O, and thus give the exciting impressions and cause an electric current in the wire, whose direction will be *from* the spectator—(see Positions 6 and 7 of the theory)—but when the permanent magnetic poles are withdrawn from the iron cylinder, N S, the electric current in the section, O, will be reversed, being then caused by those magnetic lines exterior to the wire, which, by rushing towards their native bar, give the exciting impressions.

To prevent circumlocution, it may be convenient to call the first-mentioned motion of the magnetic lines the *magnetic distention*, and the latter motion of those lines the *magnetic collapsion*. The electric currents excited by either the *expansion* or *collapsion* will continue to flow during the whole time the magnetic lines are in motion, but will cease to exist when those lines have become stationary.

By considering O the section of one side of a ring, or of one convolution of an endless helical wire, placed on the iron bar, it will be easily understood that by the *magnetic distention* the exciting magnetic lines will advance upon the *inner* surfaces of those conductors, and give impressions on every side alike; and by the *collapsion* those lines will advance upon their *outer* surfaces, again giving the exciting impressions on every side. Hence, during either a *distention* or a *collapsion*, a ring receives more exciting impressions than a straight wire, and a helix more than a ring, whilst the same law holds good in every case.

Hitherto the application of the proximate laws of Magnetic Electricity appears exceedingly simple, whether the exciting magnetic lines be permanent or transient; but, before we proceed to the application of those laws which are not so easily perceivable, it will be necessary that some explication of them be given in the simplest experimental process in which they are found to operate. By the proximate laws of Magnetic Electricity alone we can account for the production and direction of an electric current in a helix inclosing an iron bar, by the conversion of that bar into a temporary magnet; but those laws do not furnish us with means sufficient to comprehend the converse fact—viz. that a similar current in the helix, from any other source of electric action, would convert the iron into a magnet whose poles would be the reverse of the former; nor do I know that this fact is much known—certainly never attempted to be explained. It was first noticed by Dr. Faraday.

If the electric current and the magnetic matter of ferruginous bodies operated on each other by *direct* action in the production of these phenomena, a direct reciprocity of excitation would certainly be expected; but since experience shows that this is not the case, the discovery of an intermediate agent, if any there be, becomes desirable and important. This agent has long appeared to me to be the Magnetism of the conducting wire, whose laws of action I will now endeavour to develop. I shall consider the source of electric action to be a single Voltaic pair; and as it is possible that some of my readers may not be acquainted with the phenomena, I will put them in the shape of problems, and attach to each its proper solution.

Problem.—Parallel wires, carrying electric currents in the *same* direction, attract each other; but, when carrying currents in *opposite* directions, they repel each other. Why these phenomena?

Solution.—Every electric current is productive of, and enveloped by, a magnetic atmosphere, extending outwards to various distances, according to various circumstances connected with the sources of the current and the nature of the conductor. The particles constituting this atmosphere, like all other magnetic particles, are north and south polar, and arranged in consecutive polar order in circular planes, concentric with the axis of the current.

Let the circle, *c*, Fig. 12, Plate XI. represent a transverse section of a conducting wire carrying an electric current *from* the spectator to behind the paper: then the Magnetism of the metal, and perhaps that of the surrounding medium also, will be arranged by that current in an infinite number of curve lines, concentric with the boundary of the section, and extending to a considerable distance, in the manner shown by the figure.

Let now these magnetic lines, or the magnetic force they represent, be compounded into four *tangential* lines, as shown round *c' c*, Fig. 13; and let the *tangential* lines round *z* also represent the resultants of the magnetic force surrounding the section of

another current, flowing in the opposite direction to the former: then, by contemplating the character of those magnetic lines which are the most contiguous to each other in these three sections, we find that round those wires, c' c, carrying currents in the *same* direction, the vicinal lines present *dissimilar* poles to each other. Hence, by the laws of magnetics, they will have a tendency to *approach* each other; but, by observing the magnetic arrangement about those currents which flow in *opposite* directions, as in c' and z, it is found that *similar* poles are presented to each other by the contiguous lines situated between the wires, on which account the tendency is to recede from each other.

The magnetic lines thus *immediately* produced by an electric current are the primary or true electro-magnetic lines—those about a soft iron bar, magnetized by Electricity, being a secondary production.

Problem.—In what manner do the primary electro-magnetic lines operate on a ferruginous bar, inclosed in a spiral conductor?

Solution.—The electro-magnetic lines operate on the latent magnetic particles of iron in precisely the same manner as they are operated on by the magnetic lines of a permanent steel magnet, arranging those particles in regular consecutive polar order, according to the laws of magnetics; or in the same manner as the two magnetized bars would arrange themselves with each other, when permitted to move by their mutual tendency alone, which would be side by side with their poles reversed.

Let the small circles in Fig. 15, Plate XI. represent sections of a spiral conducting wire, enclosing a soft iron bar, N S, and let the electric current through the three sections on the upper side be carried *from* the spectator, and consequently returning *towards* him through those on the lower side of the spiral. The electro-magnetic force of this spiral will call forth the latent magnetic matter of the inclosed bar, and arrange it in magnetic lines in the reverse order of those resultants in the interior of the spiral, which, in consequence of their vicinal situation, are the principal ones brought into action; and by their operating in concert with each other, the magnetization of the ferruginous bar is promptly and uniformly accomplished. The short lines, with cross heads on the face of the magnet, and the long lines on its upper and lower sides, show the direction of the ferreous magnetic lines with reference to the extremities of the bar—a representation of the actual distribution of those lines being omitted to prevent confusing the figure.

Remark.—It will now be seen that the ferreous magnet is not a *direct* or true *electro-magnet*, as is generally supposed, but is a magnet by a *secondary* cause, or by the action of the electro-magnetic lines of the conductor. The theory of electro-magnetic lines will also explain the curious problem presented by the fact, that ferreous Magnetic Electricity and the Electro-Magnetism of ferreous bodies are not the reverse of each other. The ferreous magnet, N S, Fig. 15, would have its poles in the reverse

order, if the electric current were the *immediate* cause of its production—unless one system of laws prevailed in Magnetic Electricity, and another system in Electro-Magnetism; but by recognizing the electro-magnetic lines, the phenomena, by both processes, harmonize with each other, and are in exact accordance with one and the same system of laws which are found to prevail in the minutest and most complex ramifications of this branch of physics.

Problem.—If a wire, carrying an electric current, be approached by another (endless) wire, the production of a *secondary* electric current is the consequence; and the production of another current is accomplished by the receding of the endless wire. Why do these *secondary* currents traverse the wire in reverse directions? Why also the transientness of these secondaries?

Solution.—Let *c*, Fig. 14, be a section of a conducting wire, carrying an electric conductor *from* the spectator; and let *c'* be a section of an endless wire, placed within the magnetic atmosphere of the former, and which can be made to approach or recede from it at pleasure. When the endless wire, *c'*, approaches the conductor, it will advance on those magnetic lines of the latter which are situated between them, and thus become the channel of a *secondary* current, as decidedly and for the same reason as an electric current would be produced by its advancing on the magnetic lines of a ferreous magnet; and, according to Positions 6 and 7 of the theory, this *secondary* electric current will flow *towards* the spectator, or in the opposite direction to that of the primitive current. But when the endless wire recedes from the primitive conductor, it will advance on those magnetic lines *exterior* to it; and the *secondary* current being produced by the exciting impressions of those lines, will flow in the opposite direction to the former secondary, and consequently in the same direction as the primitive current in the conductor, *c*.

The transientness of these secondary currents will be obvious by considering that neither of them can exist any longer than whilst the wire is in motion.

Problem.—If an endless wire be placed parallel to the conductor of a Voltaic pair, a *secondary* current is produced in the endless wire at the moment of completing the battery circuit; and another *secondary* current is produced in the same endless wire when the battery current is cut off, but none during the intermediate time. Why these phenomena? Why also do these secondaries run counter to each other?

Solution.—The explanation in this case will be similar to that in application 4th. Let *c*, Fig. 14, represent the section of a conductor *ready to carry* a battery current *from* the spectator at any moment it may be wanted; and *c'* a section of an endless wire placed close beside the former, and parallel to it. When the battery contact is made with the former wire, *c*, the rushing current displaces the latent magnetic particles of this conducting wire, and arranges them in regular polar lines, which distending on every side, some of them necessarily advance on the other wire and

produce in it a *secondary* current; which, according to Positions 6 and 7 of the theory, will be in the revers direction to the primitive battery current. When the battery current is cut off from the conductor, *c*, a collapsion of its magnetic lines will take place, and those of them which are exterior to the other wire, *c'*, will give the exciting impressions, and the *secondary* current thus produced will flow in the *same* direction as the primitive battery current, and consequently in the reverse direction to the former secondary.

Remark.—It will now appear very obvious that the *primitive* electric current is not the *immediate* agent in the production of the *secondary*, although it is the primary cause. Its immediate agency is productive of the arrangement and distention of magnetic lines; which lines become *stationary* as soon as their parent current flows steady and equable, and remain stationary as long as that current is uniform, but no longer. When the *primary* current is cut off, its magnetic lines collapse and disappear, because the cause of their existence now ceases to exist. Hence the Magnetism of the conducting wire, &c. being an intermediate agent, becomes a *secondary cause* in the production of the secondary electric current. It appears, therefore, that the production of the magnetic lines by the primary current is truly *electro-magnetic*, whilst the *secondary* current, on the contrary, is a *magnetic-electrical* phenomenon. Hence also, as the very existence of the secondary current depends upon the motion of the primitive electro-magnetic lines, no secondary can possibly be continued when those lines have become stationary.

The explanation here given of the production and direction of secondary currents applies to conductors of every fashion, whether they be straight or curved, helices or simple rings; for in all cases the sections, *c c'*, may be considered as portions of any shaped conductor whatever.

Lemma.—Those effects produced by an electric current when a circuit is first completed, may be called the *initial* effects; and those produced when the current is cut off, the *terminal* effects.

Problem.—The *initial* magnetic effects of an electric current are greater than the *terminal*, and the physiological effects are in the reverse order. Why these phenomena?

Solution.—The *initional* magnetic effects are caused by the production and sudden distention of electro-magnetic lines, on every side of the conductor; by which means the needle becomes vigorously deflected, to an extent much greater than the subsequent steady, uniform arrangement of those lines will maintain it. But when the battery current is cut off, those lines immediately collapse and suffer annihilation; so that instead of deflecting the needle further than before, they cease to support its deflection altogether. The same explanation applies to the magnetizing of ferruginous bars.

The physiological phenomena, on the contrary, being the immediate effects of electric agency, and not of the electro-magnetic, require a distinct explanation.

The *initial* electric effects can proceed from no cause but that of the original source of the current, as from the action of a voltaic pair, for instance. For although this current will be productive of magnetic lines, those lines during their formation and distention *recede* from the current and do not advance upon it; hence they give no exciting impressions. They are, in this instance, no *cause* of the current's existence nor of its energy, but, on the contrary, are one of its productions. Moreover, as the physiological phenomena are displayed to the greatest advantage when the conducting wire is formed into a close coil: it may be shown that the distention of the magnetic lines will tend to produce a counter current in the conductor.

Let *c*' *c*, Fig. 16, Plate XI, represent transverse sections of two vicinal portions of the conducting wire, both ready to convey the current, by the *initial* impulse, *from* the spectator. The magnetic lines by this current will be arranged round both sections in the order seen in the figure; and during their *distention* will advance on each other's portion of the wire, giving exciting impressions for the production of a *secondary* current, which secondary, according to Positions 6 and 7, would flow in the reverse order to that of the primary battery current; and the closer those sections are together, the more effectual would be those exciting impressions. And as every section of the wire produces a similar distention of its magnetic lines, the tendency to produce a secondary current in the vicinal convolutions of a coil, becomes greater as those convolutions are more numerous and more closely packed together. This tendency to produce a counter current in the conductor, partially neutralizes the efforts of the primary battery current: hence the atony in its *initial* physiological effects. Hence also the transient deflections of a magnetic needle situated at a remote part of the circuit, become *lessened* by having the principal part of the conducting wire formed into a compact coil. (See table of transient deflections, page 297.)

When the battery current is cut off, the electro-magnetic lines, which, during their distention, tended to counteract the efforts of this current, now suddenly *collapse*, and by advancing rapidly on the moving fluid on every side of the axis of the current, give to it a new impulse in the direction of its previous motion; and the electro-momentum, thus increased by the secondary, is now enabled to overcome those resistances which the battery energies alone were not capable of subduing; and as the physiological effects are strengthened in proportion as the resistances presented by animal bodies are conquered, this class of the *terminal* effects of a current become greater accordingly. The manner of excitation, by this collapsion, may possibly be better understood by again looking at Fig. 16, where the two vicinal sections of the coil wire are supposed to be situated within each other's electro-magnetic atmospheres: the

two groups of remote polar lines c and c' being portions of those belonging to the sections c and c' respectively. When the battery connections are cut off, a *collapsion* of the electro-magnetic lines in the coil suddenly ensues, and those represented by the group, c, rush upon the section, c'; whilst those represented by the group, c', rush upon the section, c; each group producing new exciting impressions, upon the principles of Magnetic Electricity; and by Positions 6 and 7 of the theory, these exciting impressions are productive of a momentary electric current, in the *same* direction as that originally flowing from the battery. Only two sections of the coil wire are drawn in the figure, to prevent confusion in the illustration; but it is to be understood, that as the electro-magnetic atmosphere of each convolution extends to a sufficient distance to embrace several of the neighbouring convolutions, the electro-magnetic collapsion of one convolution will be productive of exciting impressions in many others which are placed near to it; on which account the electro-physiological effects are greatest when the wire is formed into a close compact coil. (See experiments, commencing at page 290.)

Problem.—The initial *secondary* currents are feebler than primitives which bring them into play, and the terminal secondaries are in the reverse order. Why these phenomena?

Solution.—A *secondary* current is the immediate production of either a distension or a collapsion of the magnetic lines of the primitive. In the first case the lines advance on the second conductor, and produce in it a current in the reverse order to that of the primitive, as has already been shown. This *secondary* current, in its turn, becomes productive of magnetic lines, whose poles, on the adjacent side, would be in the same direction as those of the primitive magnetic lines, as will be understood by looking at c and z, Fig. 13, Plate XI.; the former being a section of the primitive, and the latter a section of the secondary current. Hence the moment the secondary begins to flow by the impressions of the *foremost* lines of the distention from the primitive; magnetic lines from this feeble secondary would distend also, and meet those of the primitive, which had not yet arrived at the electric matter in the wire, *north* pole against *north* pole, and *south* pole against *south* pole, until the reacting poles of the secondary systems of magnetic lines had counterbalanced the acting poles of the primitive system. At this period there would be a virtual pause of both systems of magnetic lines; and as the very existence of the secondary current has hitherto depended upon the distention of the magnetic lines of the primary, this pause would tend to slacken the secondary current, and even terminate its existence were its slackening not attended by a partial collapsion of its own magnetic lines, and a corresponding distention of those of the primitive, by the exciting impressions of which it would be again partially recruited until another hostile meeting of the two systems had again brought all motion, excepting that of the primitive current, to a pause.

This pause would be succeeded by another partial distention of the primitive magnetic lines, which would again impel the secondary current; and by similar vicissitudes would the existence of this current be continued until the arrival of the hindermost distending magnetic lines of the primitive, which, by giving the terminal exciting impressions, would produce the final effort of the secondary, whose cause of existence having now vanished, would itself soon cease to exist. These motions and countermotions of the magnetic lines of the *secondary* would impede the progress of those of the primitive, and convert, as it were, their usual smooth, *rapid* distentions into a comparatively *slow* pulsatory one, whose exciting impressions thus impaired would be attended by a corresponding atony in the resulting secondary current.

With regard to the phenomena attending a *terminal* secondary and its primitive, the first part of the process is the suspension of the battery action, which will be attended by a collapsion of the primitive magnetic lines; and those of them which were situated exterior to the wire of the secondary will advance upon it, and give the exciting impressions which bring the secondary into existence. The production of this secondary will be attended with a distention of its own magnetic lines, whose poles will be arranged in the same order as those in the primitive. Those magnetic lines of the two systems which are situated between the wires will present poles of opposite kinds to each other, by the influence of which the two wires, if free to move, would be drawn together; but as the wires are fixed, the magnetic lines alone will approach each other with great celerity. This attraction will be attended with a partial retardation in the subsequent part of the collapsion of the primitive's magnetic lines, and a more free distention of the secondary. The former effect will lessen the usual magnetic impulse on the primitive current, and the latter will tend to produce a *counter* current in the wire of the primitive. Hence, on both these accounts, the terminal effects of the primitive will be much abated, if not completely annihilated. The secondary, on the contrary, being once brought into play, has nothing to obstruct its motion; and the free collapsion of its own magnetic lines, giving it another impulse, will enhance its original effects as decidedly, and for the same reason, as a battery current is exalted when no secondary conductor is present.

The phenomena of *secondary* electric currents unquestionably present the most complex problems of any in either Electro-Magnetism or Magnetic Electricity; and I have selected those for solution which appear more difficult of explanation than any other with which I am acquainted, as a test for the correctness of the theoretical principles which I have advanced.

W. S.

London, *May*, 1837.

SUPPLEMENT TO THE SIXTEENTH MEMOIR.

ON THE PRODUCTION OF SECONDARY ELECTRIC CURRENTS IN A METALLIC SPIRAL, INDEPENDENTLY OF OPENING AND SHUTTING THE BATTERY CIRCUIT; OR OF GIVING MOTION TO EITHER THE PRIMITIVE OR SECONDARY CONDUCTING WIRES.

Secondary electric currents have hitherto been produced by two distinct processes, very different from each other. One of these processes requires motion of either the *primitive's* or *secondary's* conducting wire, or of both at the same time; and the other requires the sudden opening or shutting of the primitive battery circuit.

The phenomena exhibited by these two processes of excitation, though perfectly identical, for a while appeared untraceable to any definite cause; and had not the laws of Magnetic Electricity been previously developed, and obtained an intelligible aspect, it is probable that the doctrine of secondary electric currents had not yet been very well understood; but by keeping in view the laws which govern magnetic electrical excitation, it was not difficult to trace secondary electric currents to a similar source of production. I have endeavoured, in former papers in the *Annals of Electricity*, to simplify these laws, as far as they appear to me to be susceptible; and, if I mistake not, they are now, as far as they proceed, in as intelligible a form as any system of laws that has hitherto appeared within the precincts of experimental science.

At the time I was arranging the experimental problems for solution, by the application of my theory of Magnetic Electricity, it occurred to me that, if the views which I had taken were correct, secondary electric currents ought to be produced independently of either a motion given to the conducting wires, or of opening and shutting the battery circuit; for since the phenomena of secondaries depend upon the motions of the electro-magnetic lines of the primitive battery current or its conductor, and as those lines may be put into motion in a variety of ways without any sudden disruption of the primitive circuit, by merely *varying the degree* of the battery's action, it only required the selection of the easiest plan of accomplishing the latter point, in order to proceed at once to an experiment—which, if successful, promised to be more important than any other yet on record in supporting the doctrine of secondary electric currents, which I was then presenting to the notice of philosophers.

The first apparatus employed in this investigation consisted of two concentric coils of wire, one above the other, on the same reel—a Galvanometer, and a Voltaic battery of a single pair. The copper and zinc plates of the battery were each six inches high and four inches broad, with conducting wires attached by solder, to connect them with the inner coil. The trough which held the plates and acid solutions was twelve inches long and sufficiently wide for the free motion of either plate parallel to its own plane, from one end to the other. The zinc plate was fixed by slips of wood, close and parallel to one end of the trough, which was filled to a little less than four inches deep with very dilute nitrous acid. The ends of the inner coil-wire being connected with the copper and zinc plates, and the ends of the outer coil-wire with the Galvanometer, the experiments were carried on in the following manner:—

On plunging the copper plate into the acid solution at the distance of six inches from the zinc, the battery current rushed through the inner coil, and the Galvanometer needle was deflected by the secondary produced in the outer coil, according to the law which governs the excitation by a *distention* of the electro-magnetic lines of the battery current. When the needle had come to rest again, the copper plate was advanced towards the zinc, until within half an inch of the latter metal. The needle, during this motion of the copper, was again deflected in the same direction as before, indicating a flow of a current through the Galvanometer, and consequently through the outer coil. The copper plate was now made to recede from the zinc to nearly the opposite end of the trough, during which the needle deviated on the other side of the meridian, indicating the flow of a secondary current in the opposite direction to the former. By timing the motions of the copper plate, to and from the zinc, to the motions of the needle, the latter was made to sweep over an arc of 40° on each side of the meridian.

The results of these experiments were in strict accordance with my anticipations, and were perfectly satisfactory as far as regards the production of secondary electric currents by periodic vicissitudes in the energy of the primitive. To understand the mode of excitation of these secondaries, it is only necessary to bear in mind that the electro-magnetic lines of the primitive are never stationary only whilst that current is of an invariable energy, and that every vicissitude of the latter is attended by a corresponding motion of the former. When the power of the battery current is exalting, by the gradual approach of the copper to the zinc, the electro-magnetic atmosphere of the inner coil is distending, and gives the exciting impressions to the exterior coil; but as the copper plate recedes from the zinc, the battery current becomes gradually feebler, and a corresponding *collapsion* of its magnetic lines takes place. Exciting impressions are now given to the exterior coil, producing a secondary current in the opposite direction to the former.

Notwithstanding these satisfactory results, the battery I employed was by no means well adapted for the purpose of the experiment. The wave of acid solution, produced by the motions of the copper plate, would frequently flow over the side of the trough, and cause a nuisance on the table it was placed on; besides which, the trough itself was too cumbersome for a lecture table apparatus, and the power of the secondaries, by this process, were not sufficient for prompt illustration to an extensive class.

The battery I now use for exhibiting secondary electric currents, by varying the energy of the primitive, consists of two long, narrow concentric cylinders of copper and zinc—one of each metal; the zinc, being the inner one, is covered with calico to prevent its touching the copper, in which it is fixed by wedges or otherwise. To the top of each cylinder is soldered a long copper wire, for connections with the inner coil of the reel. The cylinders are placed in a glass jar, ten inches high and about two in diameter.

When the connections are properly made—the inner coil with the battery, and the exterior one with the Galvanometer—the jar is to be nearly filled with acid solution. This done, the battery action is lessened by lifting the metallic cylinders gently upwards, exposing a less and less surface to the acid solution. By this means a secondary of considerable deflecting power is brought into play. The opposite secondary is produced by letting the metals down again, and thus augmenting the energy of the primitive; and by proceeding in this manner, moving the metals up and down in the acid solution, in correspondence with the motions of the needle, the latter is soon made to sweep a very extensive arch of the card. The metals, by this process, are never permitted to quit the acid solution, and consequently the secondaries are not produced by opening and shutting the circuit; neither are they *momentary* only, as by those processes, which is a great advantage in the production of deflections, and also in that of decompositions.

Another method of producing secondary electric currents, without opening and shutting the primitive's circuit, is by means of the magnetic electrical machine, described in the twelfth Memoir; or with any other magnetic electric machine to which a similar discharging apparatus is attached. The *polar springs* of the machine (or the *polar cells*, as described in that Memoir), which are connected with the semi-wheels, Fig. 6, Plate XII. are united by wire to the extremities, *z c*, of the inner coil-wire, Fig. 9, Plate XII.; and a Galvanometer, or other apparatus, to the extremities, *r r*, of the outer coil-wire. By turning the wheel of the machine, the usual deflections of the needle and other phenomena are produced by the secondary current.

When *interruptions* are made in the primitive, as in the method of producing shocks by magnetic electrical machines, and the cylinders *r r*, Fig. 9, are held in the hands, shocks still more powerful than those given by the machine are experienced. The bundle of iron wires has here its singular effect of increasing the power of the shocks.

London, *February*, 1838. W. S.

ON THE PHYSICAL THEORY OF ELECTRO-MAGNETISM, WITH ITS APPLICATION TO PHENOMENA.

SEVENTEENTH MEMOIR.

The principal theoretical views that have hitherto been taken respecting the production of electro-magnetic phenomena are already stated in the *Historical Sketch*, commencing at page 28; but as they differ from one another in several material points, it would perhaps be difficult, if not impossible, to select any one as the most perfect, or nearer to the true theory than any of the rest. To those who are in favour of the existence of two electric fluids, the "*conflicts*" of Œrsted might look reasonable enough, or they might reconcile their minds to the hypothesis of Ampere; but in both these views the identity of Electricity and Magnetism requires also to be acknowledged. Nor are we much relieved from this concession by the *vertiginous* hypothesis of Wollaston.

Although Mr. Barlow insists on no particular element or characteristic force being developed in the conducting wire, the hypothesis upon which he founds all his calculations and explanations of phenomena admits of a *direct* or *immediate* action between the "*particles of the Galvanic fluid in the conducting wire*" and the "*particles of the magnetic fluid in a magnetic needle*" or other steel bar. Whilst describing De la Rive's Floating Helix,* the same eminent philosopher observes that the experiment "throws great light upon the nature of electro-magnetic action, and proves most satisfactorily that, notwithstanding the intimate relation between the electro-magnetic and simple magnetic fluids, they are not identical; for no possible arrangement of simple magnets can be made that would lead one of them beyond the pole of another to find its state of equilibrium in the middle of the latter." (*Magnetic Attractions*, second edition, page 286.)

This view of the action of one magnet on another is no novelty in Magnetics, being that usually taken by philosophers from time immemorial; but it rests on very slender data, which have no place beyond certain limits of experimenting, and by no means disproves the identity of the electro-magnetic and the simple magnetic elements. The *apparent* anomaly arises from the different arrangements of the forces in the two cases, and not from any real dissimilarity of their kind. The arrangement of mag-

* Historical Sketch, page 19.

netic forces displayed by an electric current, are as uniform as the boundries of the conductor, whilst those on the surface of a bar of steel never arrange themselves after the fashion of the metal. Their apparent complexity, however, is easily annalysed and reduced to a degree of simplicity that enforces a recognition of their identity with those surrounding an electric current.

If, for instance, the curve line of force, N E S, on the surface of the magnet, N S, Fig. 6, Plate XIII. be resolved into four rectilineal forces, two of which are opposite the *north polar region*, and the other two opposite the *south polar region*, those lines of force which are *parallel* to the axis of the magnet will observe one uniform polar direction; but those which are *perpendicular* to that axis have their poles the reverse of each other on the two regions of the magnet.

If now the longitudinal forces alone, or those parallel to the axis of the magnets, were to be situated as represented by Fig 7, their mutual tendency would be to bring the magnets to the position represented by Fig. 8.

If, again, the lateral, or perpendicular forces, on different parts of the steel surfaces, A and B, Fig. 9, were to be represented by ordinates to the curve lines of Coloumb, as in Fig. 9, then, because of the repulsion between *n* and *n*' being greater than that between *n*' and *e*, plus that between *n* and *e*', both on account of degree of force and vicinity of action, the moveable magnet, B, would be urged in the direction of the spectator's right hand, or towards *e*', the equator of the immoveable magnet, A. When the pole P, had arrived at *e*', the forces of attraction between the north polar and the south polar regions of the two magnets, would tend to carry B forward until its equator, *e q*, coincided with the equator, *e' q*', of the magnet, A, or until the two magnets had gained the positions represented by Fig. 8.

To show these effects experimentally, I place a bar magnet horizontally on a table, and another similar magnet is fixed at right angles to and at the end of a lever, moveable on a pivot in a horizontal plane above the former, the other arm of the lever bearing a counterpoise. The north pole of the moveable magnet is brought directly over the north pole of the other, as represented by Fig. 7, and in that position left to freedom of action. It immediately commences its journey over the other magnet, and eventually reposes in the position shown by Fig. 8. We have in this experiment a case in ordinary Magnetism, similar to that in Electro-Magnetism, developed by De la Rive's apparatus. It is true, however, that if the similar poles of the two magnets were placed farther assunder, without increasing the parallel distance of their axis, the result would be very different; for in that case the repulsive forces about the vicinal extremities of the steel, which do not partake of the lines of flexure distributed around the general surface of the metal, tend most decidedly to separate the magnets further apart: but this individual action depends entirely on the peculiar distribution of the force at, and about, the ends of the steel bars, and consequently is

very limited, and cannot reasonably be admitted as an objection to the identity in question.

I am well aware that the above described experiment is a novelty in Magnetics, and was not known at the time that Mr. Barlow formed his notions respecting the *supposed* peculiarity of action displayed by De la Rive's apparatus. It appears to me, however, that the phenomena in the two cases may very easily be traced to the same mode of action, and consequently to one and the same elementary force. That there is no *direct* or *immediate* action between an electric current and an external ferruginous body appears amply demonstrated by every known fact in Electro-Magnetism; whilst in Magnetic Electricity the magnetic forces are the immediate agents in calling forth, and propelling into currents, the electric element of the conductors.

This want of *direct* reciprocity between electric currents and ferruginous magnets occurs in consequence of the *return* action of currents being exercised *immediately* on the magnetic fluid of their own conductors and surrounding medium, and not on the latent magnetic fluid of the vicinal ferruginous bodies; for it has already been shown (page 316), that ferreous electro-magnets are beings of a *secondary order* with reference to the electric currents, and not the true or primary electro-magnets. Hence, if we are to look for a *direct reciprocity* of action between Electricity and Magnetism, it is in those cases where no ferreous body is employed that it is most likely to be found; and by turning our attention to the production of *secondary* electric currents, we at once perceive that harmony of action and reaction so universally displayed throughout every part of physical science. A *primitive* current is here productive of those electro-magnetic lines of force, which, in their capacity of exciters of Electricity, would produce a *similar* current in the same conductor. A more beautiful and precise display of reciprocity of action cannot possibly take place between any two elements whatever: it satisfies the most rigid philosophical demands, and will henceforth claim attention in every survey of this department of science.

Having thus disposed of the principal difficulties in assimilating Electro-Magnetism to the Magnetism displayed by ferruginous bodies, the phenomena of the former are easily explained upon the principles of ordinary Magnetism. The attractions and repulsions of electric currents, and the production of ferreous magnets, have already been explained in the preceding Memoir; it remains now to show in what manner the electro-magnetic lines of a conducting wire are employed in the production of rotatory motion. The simplest cases are those represented by Figs. 13 and 14, Plate A.

Let Fig. 10, Plate XIII. represent a horizontal section of the horse-shoe magnet, and the pendent wire of Fig. 12, Plate A, the latter being represented by the circle, c, and the current, through this section, flowing *downwards*. By this arrangement the lower end of the pendent wire, c, would be thrown out of the bed of mercury, in

which it was immersed, in the direction indicated by the small darts. This deflection of the wire breaks the contact and arrests the current, and the wire falls down again to the mercury and closes the circuit. Another impulse is now given to the wire, which is again deflected and the circuit opened; and, in consequence of a succession of these alterations, the wire is kept exhibiting its vibratory motions. If either the position of the magnet or the direction of the current be reversed, the pendent wire is impelled in the opposite direction.

To account for these motions, we have only to look at the arrangements of the two systems of magnetic force, and examine their operations on each other upon the principles of ordinary magnetic action. The four right lines, with cross heads, around the section, c, may represent four resultant tangential lines of electro-magnetic force; and the long lines (some straight and others slightly curved), with cross heads, may represent resultants of the magnetic force of the steel. (See also Fig. 4, Plate XI.) Now as the two electro-magnetic resultants, which are parallel to the two branches of the magnet, have their similar poles in opposite directions, they neutralize each other as regards their action on the magnetic resultants of the steel, and have no influence in giving motion to the wire either one way or the other. Hence the two electro-magnetic resultants which are parallel to the magnetic resultants of the steel, are those only which are influenced by the latter and tend to give motion to the wire. Then, according to the laws of Magnetics, it will be observed that, on the right hand, as the tangential electro-magnetic resultant has its poles in the reverse order to those magnetic lines of the steel which are situated on the same side of the wire, they will mutually attract each other; and as the wire is free to move, it will be drawn in that direction which is indicated by the three small darts. It will be observed also, that the two systems of force on the left side of the wire exercise a repulsive action on each other by which the wire is urged in the same direction as by the former force of attraction.

If the current were to be reversed in the wire, its electro-magnetic forces would be reversed also; and their action on the magnetic lines of the steel would tend to impel the wire towards the bend of the magnet. The same application of the theory to the wheel apparatus, represented by Fig. 13, Plate A, will explain its rotatory motions.

In applying the theory to the rotatory motions which a pendent wire, carrying an electric current, performs round a magnetic pole, little more will be necessary to be observed than the explanation already given for the vibratory motions. If, for instance, the section, *c*, Fig. 11, Plate XIII. of the wire, carrying a current downwards, or *from* the spectator, were to be situated near to the north pole, N, of a bar magnet, the magnetic lines of the latter, and the electro-magnetic lines of the former, would be arranged as in the figure; therefore, the direction in which the section, *c*, would be urged would be that indicated by the arrow. By reversing the direction of

the current in the wire, its electro-magnetic lines would also be reversed as respects the magnetic lines of the magnet, on which account the rotation round the pole would be reversed accordingly.

Besides the forces shown by Fig. 11, there are others in operation which tend to urge the inferior extremity of the wire in a *sloping direction upwards ;* and which if resolved into two forces, one perpendicular and the other horizontal, the latter component alone would carry the wire round the magnetic pole. In order to explain these motions, it will be necessary, in the first place, to understand that the lines of magnetic force concerned in their production are those of the electric current, which, in all cases, are uniformly arranged round the wire, and those proceeding *immediately* from the pole of the steel, and which do not partake of the flexure of those curve lines which envelop the principal part of the bar between its poles.

Thus premised, let *c z*, Fig. 12, Plate XIII. represent the pendent wire, hanging freely at the point, *c*, between the spectator and the north pole, N, of a bar magnet placed vertically. The electro-magnetic lines of force situated on that side of the wire nearest to the magnet, and those on its opposite side, are represented by short transverse lines, with cross-heads ; and those magnetic lines of the steel to which they are exposed are represented by the long, slightly curved lines, also with cross-heads.

Now since the electro-magnetic lines of the wire which are thus situated would neutralize each other's action with respect to the magnetic lines of the steel between which they are placed, we have next to examine the positions of those of the wire which are situated at right angles to the former, and those of the magnet. For this purpose, imagine Fig. 12 to be turned, on the axis of the magnet, through a quarter of a circle towards the spectator's right hand, so as to assume the position represented by Figs. 13 or 14. In both figures the curve lines are those belonging to the magnet, and the short, right lines are representatives of those portions of the electro-magnetic force which are efficiently employed in giving motion to the wire. Fig. 13 represents the two systems of force as they are situated *behind* the wire, and Fig. 14 the respective situations of the same systems in *front* of the wire. The former shows a tendency to move the wire *towards* the spectator by repulsion, and the latter to move it in the *same* direction by attraction. On both these accounts, therefore, the wire would be urged in one and the same direction ; but in consequence of the mode of suspension, and the slight obliquity of the lines of force proceeding from the pole of the magnet, the inferior extremity of the wire is lifted up and often thrown out of the mercury in the same direction as that in which it revolves round the pole—which, in this case, is from left to right, with respect to a spectator situated in the axis of motion, or coincident with the position of the magnet. Hence both modes of explanation amount to the same thing: they are both, however, very different to the explanation given by Mr. Barlow, but they have this advantage, that they agree with

the phenomena, whereas the hypothesis of Mr. Barlow has carried the rotatory movement in the opposite direction to that in which it is actually performed. The real direction of rotation which the wire performs round the *marked* or north pole of the magnet, when the current is *descending*, is shown by the arrow behind the N pole in Fig. 11, Plate V.* ; and that round the south pole, by a similar current, is indicated by the arrow behind the S pole.

As the rotation of a magnet round a fixed conductor, carrying a current, is performed in the *same* direction as the wire round the magnet, the same principles will not apply to both cases, otherwise a reciprocity of action would be a sufficient explanation ; but by taking into account those components of magnetic force of the steel that are employed in the deflections, the rotation of a magnet round an electric current is easily accounted for.

On the right hand side of Fig. 2, Plate V. it will be observed that the N pole of the magnet is upmost, and the current from *c* to *z* ; hence the conditions are precisely the same as on the right hand side of Fig. 11 of the same plate. The arrows in both cases indicate the direction of rotation to be the same. If the magnet in Fig. 2 were entirely at liberty to move, it would take a position similar to that of the arrow, with its N pole towards the arrow's point, and its S pole towards the feathered end of the arrow. Now since this is the direction in which the N pole actually moves, the rotation is a consequence of a continuous tendency to gain its natural position† at right angles to the axis of the wire.

If the rotation of the wire round the magnet were the result of a continuous effort to place itself in its *natural* position, at right angles to the magnet, it would be performed in the opposite direction to that which actually takes place. But it appears more like the result of an effort to *extricate* itself from its *unnatural* position, similar to that so beautifully shown by De la Rive's apparatus when the wrong pole of the magnet is presented to it. The rotating wire, however, has not that facility of motion which would enable it to turn round and approach the magnet with its current reversed, as is the case with the floating apparatus.

The vibrating wire is in the same predicament as the rotating one when it is projected outwards, or in the direction of the darts, Fig. 10 ; but when it is projected *inwards*, or towards the bend of the magnet, by the reverse current through the section, *c*, its efforts are those which, if it were perfectly free to move, would carry it onwards to its natural position at the centre of the magnet.

* This is the direction which Mr. Barlow has given for an *ascending* current ; and as the rotatory cylinder is supposed to be an assemblage of such wires, carrying similar currents, the error is repeated in explanation of its motions. (*Magnetic Attractions*, pages 275 and 269.)

† The position to which an electric current and a magnet have a tendency, with respect to each other, is here called their *natural* positions.

To apply this theory to the explanation of the rotations which a magnet performs on its own axis, when subjected to the electro-magnetic forces of a current, requires a knowledge of a fact which I produced some years ago whilst contemplating those rotations. As the rotations of a magnet on its axis are performed in the same direction as its revolutions round a fixed conductor, when the electric current is in one and the same direction in both cases, it appeared highly probable that both phenomena are produced by similar operations of the two systems of magnetic forces; and if so, then the electric current must necessarily flow along the axis of the magnet, and operate on the Magnetism of its exterior as if the latter were composed of a surrounding system of small parallel magnets, whose individual axis would be all parallel to the axis of the steel, which would not only be the axis of the magnetic system, but also coincide with the axis of the electric current. Hence, by such a system, both the current and the magnets rotated together in the same piece of metal; and if this were absolutely the case, I could see no reason why a system of magnets, fixed to a central conducting wire, should not rotate upon the axis of the whole. To put this hypothesis to the test of experiment, I fitted up the apparatus which is already described in page 103.

Having succeeded so satisfactorily with two magnets attached to the wire, I was desirous of carrying the analogy still farther by making the system of magnets more complete. For this purpose I made eight narrow steel magnets, and distributed them equally around the axial wire, by tying them against the curved surfaces of two cylinders of cork, through the centre of which the axial wire passed. The north poles of these magnets were all placed in one and the same direction, and consequently all the south poles in the other. By this arrangement I had a compound hollow octagonal magnet, whose axis was the axis of the vertical wire.

The rotations with this system were precisely the same as with the former system of two magnets. When the system is inverted without altering the battery connections, as is easily done by the apparatus represented by Fig. 15, Plate XIII. the rotation appears to a spectator to be inverted also, though in reality it still proceeds in the same direction with regard to its own poles.

From the uniformity in the results with all these differently formed magnets, it is fair to conclude that the rotations of a solid bar magnet on its axis are accomplished by a similar distribution of the Magnetism of the bar around axial currents to that in the compound systems described; and that the whole of this class of phenomena result from a series of mutual efforts between the magnetic forces of the steel, and those of the conducting wire or its currents, to balance one another; or, in other language, from a mutual tendency of the magnet and the electric current to place the axis of the latter in the plane of the equator of the former.

It is now some years since I first published my views respecting the pulsatory character of electric currents; I now find that others are entertaining similar notions

By admitting electric currents to be pulsatory, we should have another element to assist us in the explanations of electro-magnetic rotations; but I have not availed myself of that element lest it should not yet be generally admitted.

W. S.

Royal Victoria Gallery of Practical Science,
Manchester, April, 1841.

ON THE IGNITION OF GUNPOWDER BY THE ELECTRIC DISCHARGE; AND ON THE TRANSMISSION OF ELECTRICITY THROUGH WATER, &c.

EIGHTEENTH MEMOIR.

It is generally admitted that the present state of knowledge relative to the phenomena of Electricity is enveloped in much obscurity; and perhaps no instance of electrical action manifests our ignorance of this branch of science more than that of igniting gunpowder, yet so little notice is taken of this isolated fact that no satisfatory attempt at explanation, that I am aware of, has ever appeared in the pages of any writer on this subject.

That gunpowder has frequently, by various individuals, been ignited by the electric fluid is a truth that cannot be denied; but *why* those experimenters happened to succeed, and *why* others so frequently and still more constantly *fail*, are circumstances the cause of which has hitherto been left unexplained—perhaps not understood. I am well convinced that no individual experimenter has been more embarrassed than myself by fruitless attempts to ignite gunpowder by the electric fluid; and although I have varied the experiment according to all the directions I could either read or hear of, yet I candidly confess that I never succeeded by any of them.

Fruitless as these experiments were with respect to the object in view, it was observed, by passing the discharge of a jar through water (which is the method given by some authors), that the report is very trifling when compared to that which is heard by a similar discharge through metal. Convinced by this circumstance that the nature of the discharge is *modified* in a peculiar manner by passing the fluid through water; yet as I had never succeeded in igniting gunpowder by a discharge through the aqueous medium, it appeared evident that something more than this fact was necessary to be understood; I therefore became desirous to ascertain, if possible,

the real cause why other experimenters succeeded, and why every attempt that I had made proved unsuccessful.

It is well known that if a red-hot iron be applied to gunpowder, the latter does not immediately ignite, but that some interval of time (however small) does certainly elapse before one single grain is on fire; and that it is possible for a red-hot iron to be passed over the hand with such velocity as to produce scarcely any sensation of heat. Hence my first object now was to devise some means of retarding the velocity of the electric fluid, for I considered that if this could be accomplished, more time would be afforded for the fluid and gunpowder to be in contact, and the latter in consequence more likely to be ignited.

I likewise supposed that if the electric fluid be retarded by being transmitted through water, it was likely that a jar would not be quite discharged by a very sudden contact, as it was probable that if the discharging rod was quickly withdrawn from the knob of the jar, that the whole of the fluid would not have time to make its escape. But several trials in this, through a large tub of water, seemed to discharge the jar as effectually as if the whole circuit had been of metal.

Although these experiments were by no means satisfactory, yet I always observed that the report was much feebler, and that gunpowder placed in the circuit was not blown or scattered to so great a distance, when the discharge was made through water as when a similar discharge was transmitted through metal. Hence it was obvious that the force had by some means been abated, but whether by retardation or by some change in the physical character of the electric fluid, I was at that time unable to determine.

Some time afterwards, an idea suggested that if the electric fluid be retarded at all by passing through water, perhaps the water possesses this property in consequence of its inferior conducting capacity with respect to metals and other good conductors; and if so, the velocity of the electric fluid might be reduced to almost any degree by reducing the diameter of the column of water through which it had to pass. For it is evident that the conducting power of any body will be proportional to its natural capacity, and to the quantity employed at any one point in the circuit; for a discharge that will destroy a thin wire, would be conducted with safety by a wire of the same kind of metal of greater dimensions. It now occurred that those persons who had ignited gunpowder by the electric fluid, perhaps succeeded by using very narrow tubes filled with water—(for I had frequently transmitted a discharge through a wide tube without success; and as no author gives any dimensions of the water employed, it did not, till now, occur that the time of transmission would vary with the calibre of the tube). I had not, at this time, any narrow tubes in my possession. Considering, however, that if any non-conducting substance—such as silk or paper—were moistened with water, that those substances could have no more conducting power than what was imparted to them by the moisture. My first experiment was with a single

thread of sewing silk, about four inches long, well moistened by drawing it between my lips. This thread was made a part of the circuit between the inside and outside of a charged jar. At another part of the circuit an interruption was made between the extremities of two wires, and at this interruption was placed some gunpowder: on discharging the jar the gunpowder ignited. I repeated the experiment several times with the same success. I afterwards varied the experiment by using the same thread and a smaller jar, and succeeded in igniting gunpowder with about thirty inches of charged surface. I must here observe, that when the thread was very wet I never succeeded with this small jar, owing, as I suppose, to the quantity of water contained in the thread being too great to retard the small quantity of electric fluid contained in the jar. For, by squeezing out some of the moisture, the thread became a worse conductor, and then I always succeeded.

I next tried how far it was possible to succeed with the first jar, and augumenting the quantity of water. For this purpose a piece of twine was used, well soaked in water. This twine, however, conducted the electric fluid with too much facility to ignite the gunpowder; but when some of the moisture was squeezed out, it answered very well. Thus by proportioning the one with the other, I always succeeded: the same results were obtained by using moistened paper.

Having satified myself on this point, I next endeavoured to acertain if the electric fluid undergoes any change in its physical character by passing through the water; or if the ignition of gunpowder depends entirely on the time occupied by the fluid to pass through it. For this purpose I employed two jars, which, for distinction, may be called A and B. I charged A positively and B negatively, and connected their outsides by water. On exploding A into B, through water, both jars became neutralized. A was again charged positively, and afterwards partly discharged, through water, into B. On discharging both jars separately, there appeared no difference in the explosions. A was once more charged positively, and again partly discharged, through water, into B. On discharging B through a moistened thread, gunpowder was ignited in the circuit. These experiments were reversed by charging the jar, A, with negative instead of positive Electricity, and the results were similar: hence I concluded, that the ignition of gunpowder, by the electric fluid, depends on the time occupied by the latter in passing through it, and not on any change in the physical character of the fluid. Hence also, by the foregoing experiments (when the intensity of the charge is constant), the time occupied by any given quantity of the electric fluid, in passing any one point in the circuit, will be in some reciprocal proportion* to the thickness of the column of water employed in that circuit.

* This proportion may vary either as the diameter, or as the square of the diameter of the column—according as the electric fluid occupies the surface, or the whole body of the water; and if the velocity of each individual particle of the electric fluid be effected, the transmission of the whole mass will also vary on that account.

It was observed in these experiments, that the moistened thread soon became nearly dry: hence the electric fluid had either decomposed the water, or caused it to evaporate. The former effect I suppose to have taken place, which, if true, perhaps the decomposition of water by this agent may be facilitated by reducing the diameter of the column employed. I have not yet had time to ascertain this particular satisfactorily, therefore it remains a mere supposition.

I have frequently discharged a jar through my own body without any other inconvenience than a burning sensation at the extremities of the fingers, and have ignited gunpowder in the same circuit.

So modified is the electric discharge by being transmitted through aqueous conductors, that the effect of an intense charge of the most powerful battery may be reduced to almost any degree. I have discharged eight feet* of charged surface through my own body without feeling the least shock, but the burning sensation was very severe.

In medicine this modification of Electricity can hardly fail to be useful; for it may be administered to any particular part of the body without effecting any other part. It may be applied to the skin of the most delicate patient; and without the least danger of giving a shock, a most powerful stream of Electricity may be poured on the part affected.

It has been also ascertained, that the force of an electric charge through metal is always proportioned to the thickness of the wire through which it is transmitted; or that the same quantity and intensity of electric fluid acts with a greater force when transmitted through a thick than through a thin wire.

This law of course has a limit; for if the conducting wire be sufficiently stout or capacious to transmit the fluid, without interruption, a wire of larger dimensions can give no more facility to the transmission. I am of opinion, however, that thick wire facilitates the transmission of the electric fluid to a greater degree than is generally suspected.

Now it is evident that as thin wire has the property of diminishing the intensity of an electrical discharge, the fluid, during its transmission through such a wire, must necessarily be drawn out, as it were, into a longer stream than if it were transmitted through a thick one; in the same manner that a certain quantity of water would be drawn out into a longer stream by passing through a narrow than through a wide tube, or as a piece of metal would be drawn into a longer wire by passing through a small than through a large hole in the plate.

Hence it became a curious question—What length of wire, of a given dimension, does a certain quantity of the electric fluid (say a jar charged to a certain intensity) occupy at any moment during its transmission? I supposed that this might probably

* Eight feet of lining, and eight of coating.

be ascertained by placing some gunpowder at an interruption near to the positive side of the jar, and the moistened thread near to the negative side, having a long copper wire between them: for if the wire was of sufficient dimensions to contain all the fluid at once, the latter would not meet with any resistance till it arrived at the moistened thread; and the time of its transmission through that part of the circuit where the gunpowder was placed would not be lengthened—upon which supposition the gunpowder ought not to be ignited.

With such arrangements I have separated the gunpowder and moistened thread by a copper wire, No. 16, of different lengths, from one yard to twenty yards; yet with all this length of good conducting substance *beyond* the gunpowder, the latter never failed to be ignited.

I consider this a very curious circumstance, and the inquiry important; not so much because the gunpowder always ignited in these experiments (for it is possible that with longer and stouter wire, and a smaller charge, the ignition might not take place), but because if it could be proved by experiment that the electric fluid would, by intervening capacious good conductors, ignite powder at the negative, and not at the positive side of the moistened thread, such experiment would prove to demonstration the truth of the Franklinian hypothesis.

I am well convinced that if the electric fluid ever passed the gunpowder without interruption, the latter could not be ignited by any recoil of the fluid into the jar from the interrupted moistened thread; because, if ever it passed through the gunpowder with voilence, it would scatter or blow the latter substance away, so that none would remain in the circuit to be ignited at the time of the fluid's return.

By employing either a moistened thread or a narrow tube of water, gunpowder may be ignited at several interruptions in the same circuit. I have frequently passed the fluid through my own body and fired six guns by one discharge of a jar; and so instantaneous is the ignition at the several guns, that their united reports appear like the report of one gun only.

W. S.

Artillery Place, *Woolwich*, *May* 15*th*, 1826.

ON THE INFLAMMATION OF GUNPOWDER AND OTHER SUBSTANCES BY ELECTRICITY, &c.

NINETEENTH MEMOIR.

Shortly after the original publication of the preceding Memoir on the ignition of gunpowder by Electricity, I had an opportunity of perusing a paper, on the same subject, by Mr. Woodward;* but I have found that, like all other authors I have yet read, Mr. Woodward has left us entirely in the dark with respect to the *law* necessary to be observed in varying the experiment; for although the dimensions of his jar and tube are given, no mention whatever is made why it is necessary to observe those dimensions, or why other jars and tubes of various other dimensions might not answer as well. Indeed so little does Mr. Woodward's hypothesis concur with the conclusions deduced from my own experiments, that I find his supposition, that water derives its retarding or resisting property *solely* from its confinement in glass tubes, is wholly without foundation. Mr. Woodward states, that "if the tube be filled with æther or alcohol, and placed in the circuit, the powder will be inflamed. If it be filled with sulphuric acid, which is a better conductor, the powder will be scattered and not inflamed; but the dispersion will not be so great as when metals only form the circuit. The same effect will be produced by transmitting the charge through the animal economy, or through water *not* inclosed in tubes—in which case the water does not appear to oppose a sufficient resistance to the passage of the fluid."

It is evident, from this statement of Mr. Woodward, that he never varied the experiment by employing tubes of various diameters: his experiments being with different fluids in the *same* tube. Had that gentleman taken into consideration the probability of meeting other results by varying the diameter of the tube with the extent of the electric discharge, and persued his experiments accordingly, I imagine he would have found little difficulty in igniting gunpowder by transmitting the electric fluid through sulphuric acid; it being necessary to observe nothing more than to augment the charge, or to reduce the diameter of the tube, in order to produce that effect.

That the retarding property of water does not depend on its confinement in tubes, is evident from the success of my experiments with the moistened thread exposed to

* Mr. Woodward's paper was pointed out to me by the Editors of the *Philosophical Magazine*. It appeared at page 283 of vol. vii. of the *new series* of the *Annals of Philosophy*.

the open air. Indeed so certain it is that confinement does not impart to water this property, that gunpowder may be ignited in the electrical circuit by transmitting the fluid through a sufficiently narrow streak of water drawn by a pen on the surface of a piece of flat glass.

I should, however, be extremely sorry to detract any thing from the merits of Mr. Woodward; for although his explanation of igniting gunpowder by the electric fluid, when transmitted through water, does not exactly agree with the principles on which I suppose the action to depend, I nevertheless should be wanting in candour were I not to express my sentiments on the highly interesting nature of his experiments; and I feel a pleasure in acknowledging that Mr. Woodward has preceded me in some that are detailed in the preceding Memoir*—viz. those in which gunpowder was ignited on both the positive and negative side of a long conducting wire.

Mr. Leuthwaite has likewise conducted a number of interesting experiments on this subject but I believe they were intended chiefly to ascertain the *conducting power* of different fluids, in order to make choice of the most eligible for the purpose of igniting gunpowder by electric discharges. Hence so far it appears that the idea had not been entertained, prior to the institution of my experiments, that the diameter of the tube and the nature of the electrical charge would in any way affect the result of an experiment. Neither does it appear that the ignition of gunpowder, by means of transmitting the electric fluid through *unconfined* water, was ever attempted prior to the date of those experiments which are detailed in the preceding Memoir.

Whenever an electric discharge is transmitted through water for the purpose of igniting gunpowder, the *length*, as well as the diameter, of the aqueous column must always be attended to, and ought, in all cases, to exceed the *striking distance*; for if the wires which enter the extremities of the column are brought too near each other, the electric fluid will dart from one to the other with very little interruption, and in all probability will scatter rather than ignite the powder.

To insure success, the column or train of water, or any substance containing it (such as wood, twine, silk thread, paper, &c.) should never be shorter than six inches; and the thickness, or quantity of water contained in a transverse section, must always be regulated by the extent of the electrical charge employed—that is, to the quantity and intensity of the electric fluid transmitted. When a small jar is used for this purpose, the strip of water must be very thin or narrow: if a large jar be used, the thickness of the aqueous column may be increased, provided the intensity be the same as that in the former case. When the same jar is used with charges of different degrees of intensity, the lowest intensity requires the thinnest strip or column of water; not because the intensity is lowest, but because the quantity of fluid is less: and although a column of water that answers for a low intensity will also answer for every

* I was not aware of Mr. W.'s experiments till after that Memoir was published in the *Philosophical Magazine*.

intensity that is higher, a column that would answer very well for a high intensity might be far too capacious to insure success with an intensity that is much lower.

As glass tubes are both convenient and elegant for regulating the diameter of a column of water, about eight inches of barometer tube, one-tenth of an inch diameter, answers the purpose very well with any jar exposing more than half a square foot of coated surface on each side, provided the intensity of the charge be above 50° per quadrant electrometer.*

By employing a gass tube of the above dimensions, filled with water, and a jar exposing 120 square inches of coated surface on each side of the glass, gunpowder was ignited in the circuit at every trial with any intensity above 20°. When two such jars were employed at the same time, gunpowder ignited with every intensity above 10°; sometimes with an intensity as low as 7° or 8°. The gunpowder was fine-grained and of the best quality. If the grains be bruised to a fine powder, its ignition is more certain when the low intensities are discharged through it.

When moistened thread, twine, or wood is used, any of them may be cut in two and their parts separated a little, and the gunpowder will take fire at this interruption of the aqueous part of the circuit. Thus we learn that it is not necessary for metallic conductors to be in contact with the gunpowder in order to accomplish its ignition by the electric fluid.

A chain may be made of alternate links of copper wire and tailor's thread, in such a manner as not to be easily distinguished from one that is all of metal. If such a chain be dipped in water, and afterwards made part of an electrical circuit, gunpowder may be ignited at any interruption of that circuit as decidedly as though the threads were of one continued piece.

When one of the above-mentioned jars was employed, and a copper conductor formed the circuit, æther was fired, at an interruption, with every intensity above 20°. When the water tube formed part of the circuit, æther could not be fired, even though both jars were employed together, and discharged from an intensity of 90°.

When four such jars were charged to the highest possible intensity, and discharged through the water tube, gold leaf placed in the circuit was powerfully attracted but not deflagrated.

One jar, with an intensity of 60°, produced intense magnetic polarity in a sewing needle that was placed in a spiral wire which formed a portion of a complete metallic circuit. With two jars charged to the highest possible intensity, no magnetic effect was produced when the tube of water formed a part of the circuit.

* As quadrant electrometers do no afford uniform measures of electrical intensity, but differ from one another according to the weight of the ball, length of the lever or index, delicacy of suspension, &c. an intensity is here given which is expected to answer with the generality of them.

How do these experiments accord with the doctrine of *quantity* and *intensity*? By the Voltaic process a needle may be powerfully magnetized by a single pair of small copper and zinc plates; by which process it is said that we have *quantity* but not *intensity*. By the experiments I have just now described, it appears that *quantity* has not the power of magnetizing needles without it possesses *intensity*: for when the electric fluid was not retarded by inferior conductors, a very small *quantity* produced the effect; but when the *intensity* of the discharge was reduced, although three times the former *quantity* at least was employed, not the slightest magnetic effect was produced. The truth is, we appear to want another more appropriate term in Electricity—that term is "*Momentum.*" *Intensity* answers very well to express the relative degrees of concentration on the surface of jars, &c.; but *momentum* is the proper term when the fluid is in motion. Hence then, although the less quantity of fluid produced on the needle an effect which the greater quantity was unable to accomplish, this effect was probably owing to the *momentum* of the former exceeding that of the latter.*

When tow or cotton is moistened with spirit of turpentine, or mixed with powdered resin, it will ignite by a very small electrical discharge when the whole circuit is of metal; but if the tube of water form a part of the circuit, the tow or cotton, prepared as above, will not ignite, although two of the before-mentioned jars, charged to the highest possible intensity, be discharged through the circuit.

When a narrow strip of gold leaf was placed between two pieces of window glass, and made to form part of a complete metallic circuit, one jar, with an intensity of 80°, completely exploded the gold leaf. When two such jars were employed at the same time, and charged to a still higher intensity than the former jar, and the water tube entered into the circuit, a similar strip of gold leaf subjected to the discharge remained uninjured.

When gunpowder and gold leaf were subjected, at the same time, to a discharge similar to the above, and the whole circuit metallic, the latter substance was completely exploded, but the former scattered and blown away without ignition. When the water tube formed a part of the circuit, and every other part arranged as before, the gunpowder exploded, but the gold leaf remained undisturbed by the discharge of the jars.

Similar experiments were made with gunpowder and needles, gunpowder and æther, gunpowder and tow prepared as above. When the circuit was completely metallic, the needles were magnetized, the æther and tow, &c. were fired, but the gunpowder was in no instance ignited. When the water tube formed part of the circuit, the gunpowder ignited, but the other substances remained unaffected.

* These were the views I entertained at the time I was carrying on the experiments. The subject will be farther treated of in the next Memoir.

Hence it may be inferred, that in order to magnetize pieces of steel, to explode metals, to ignite æther, or tow with resin, &c. by electric discharges, *quantity* and *velocity*, or *momentum* of the fluid, is required; but to ignite gunpowder, *quantity* and *time* are indispensible—that is, when the *quantity* is constant, to accomplish the former effects, *velocity* is required; but to produce the latter effect, *time* is an essential element.

W. S.

Woolwich, November, 1826.

AN INQUIRY INTO THE ATTRIBUTES OF THE GALVANOMETER, AND HOW FAR ITS INDICATIONS MAY BE DEPENDED UPON IN ELECTRO-DYNAMIC RESEARCHES.

TWENTIETH MEMOIR.

The diversified capacities in which the Galvanometer has found employment in the service of experimenters, and the interesting discoveries which have been attributed to its assistance, have conferred on it an importance of no ordinary kind. It is an instrument now extensively employed in the contemplation of almost every class of phenomena exhibited by the agency of electric currents; and the deflections of its needle have been regarded as infallible indications of the *degree* of electric force in its wires—of the *absolute quantity* of the electric fluid transmitted by them, and of the extent of electric agency in the production of chemical action.

That the deflections of the magnetic needle are strictly indicative of the electric *force*, or electro-dynamic action, when contemplated abstractedly and independently as regards the elements of its constitution, is a truth duly acknowledged and sanctioned by the highest scientific authority; but we have not yet been made acquainted with any experiments, nor with any train of reasoning, from which we could infer that its powers of investigation have been any farther established.

There are two distinct kinds of magnetic deflections for the admeasurement of electro-dynamic action, which may very conveniently be designated *permanent* and *transient*. By the former we mean all those deflections of the Galvanometer needle, which are *constantly* maintained for some considerable time, constantly marking some

determinate angle with the conducting wire, according to the extent of the electric force which keeps the needle out of its natural direction; and by the latter we mean those momentary deflections, or first ranges of the needle, which are the effects of sudden electric discharges operating for some definite interval of time—the angle indicating the extent of the electric force being that marked by the extreme range of the needle.

These two kinds of magnetic deflections are perfectly distinct from each other, and indicate the value of electric forces by very different laws well known to scientific men, some of whom have successfully applied them to particular electro-dynamic investigations. It still, however, remains a subject of interesting inquiry, whether or no the initial and subsequent electric forces observe an uniform analogy with regard to each other; or if, in all cases of excitation, any constant ratio of those forces be indicated by the *transient* and *permanent* deflections of the Galvanometer needle whilst under the influence of electro-dynamic action. If such analogy were to be established, it would be a means of abridging our experimental labours and satisfying many of our inquiries by a simple process of calculation alone. We might then substitute, as it were, one experiment for another, and become acquainted with the values of electric forces emanating from various sources of action to which we had no immediate means of access, or if we had, it would be unnecessary to resort to them.

If, for instance, it were discovered that the forces indicated by a series of steady deflections, at intervals of one degree, through a quadrant of the graduated card, observed a constant ratio with regard to their respective initial forces, acting for some small interval of time, a law would thus be established by means of which the values of either the initial or the subsequent permanent forces might easily be calculated.

We presume not, however, from the result of our own limited investigations, to hold out even a hope of arriving at conclusions of this high importance to electrical science; but as *substitute* experiments, implying such analogy, appear to have found admission into some electro-dynamic researches hitherto held in considerable repute, and from which circumstance others might possibly be assumed, our conclusions, whatever they may be, if drawn from a well conducted inquiry, may probably be of some value to those who pursue similar paths of research, either by leading to truth or preventing error.

In the contemplation of the electric forces indicated by *permanent* deflections, the resistance which might be presented to a needle in motion, by the surrounding air, may be entirely disregarded; because, whilst under the influence of the electric force, the needle is supposed to be in a perfect state of repose. The only force, therefore, with which the electro-dynamic action has to contend is that arising from the propensity with which the displaced needle is endued of resuming its former or

natural position; and when the same Galvanometer is employed, this directive force is invariable, or it is a constant quantity, which may be referred to unity.

The deflecting force exhibited by the conducting wire during the flow of an electric current is well known to operate at right angles to the axis of that wire, around every part of its surface; or in the direction of a tangent to any point of the curve circumscribing a transverse section of the wire.

The efforts, therefore, of that force to turn a magnetic needle placed parallel to, and either above or below the wire, will be in the direction of a tangent to the horizontal arc of deflection; and the force has very conveniently been estimated in terms of the tangent to that arc, being constantly proportional to it.

If, therefore, the angle of *permanent* deflection be denoted by Δ, then the force in the conducting wires, producing that angle, will be proportional to tan. Δ; and by employing F as a representative of the force emanating from the conducting wire, we have

$$\text{Tan. } \Delta = \text{F.}^* \quad . \quad . \quad . \quad \text{(a)}$$

Electric forces, indicated by *transient* deflections of the needle, are but of very short duration, and in some cases may be regarded as momentary impulses, producing velocities in the needle's motion proportional to their intensities. If, then, the maximum velocities were to be produced at the lowest point of the measuring arc, or whilst the needle and conducting wire were parallel to each other, the electric forces, producing these deflections, would be calculated with tolerable precision, upon the principles of the doctrine of falling bodies, the deflections of the needle being assimilated to pendulous motions. It appears, therefore, an exceedingly desirable object in calculating the values of transient electric forces, that the velocity should be a maximum at the lowest point of the measuring arc: hence, in experiments of this kind, every available means should be resorted to in order to attain the greatest degree of precision in that particular. It would be exceedingly difficult, however, if not entirely impossible, to produce a maximum velocity at the precise zero point, which could be accomplished by no other means than by adjusting the needle, previously to the discharge, some few degrees backward, or on the opposite side of the meridian line to that of the measuring arc. Perhaps two or three degrees might answer as well, if not better, than any other distance for electric forces generally of about half a second duration—though much would depend upon their intensity and other circumstances, which precludes the accuracy of any general rule. For our present purpose, however, we consider this mode of experimenting sufficiently accurate.

We now consider the needle as leaving the conducting wire, or lowest point of the measuring arc, with a maximum velocity, and consequently with the same velocity as

* See Mr. Barlow's Magnetic Attractions, second edition, page 238.

that which it would acquire whilst returning from the highest point to its parallelism with the wire again, by that force alone which tends to restore it to its natural position.

Under these circumstances, the electro-dynamic action for any short definite interval of time will be proportional to the chords of the arcs of deflection, or to twice the sines of half those arcs. Denoting, therefore, the arc of *transient* deflection by δ, and the electric force producing that arc by F', we shall have

$$2 \sin. \tfrac{1}{2} \delta = F' \quad . \quad . \quad . \quad . \quad (b)$$

Electric forces producing *permanent* deflections may be regarded as if emanating from an equable flow* of the electric fluid through the conducting wire in each case, from the first moment the needle marks a steady angle; and that an equal quantity is transmitted in each equal interval of time. If, therefore, the flow of the currents were equable from the first closing of the circuit, it is obvious that the force would be constant from that moment—in which case the initial forces indicated by *transient* deflections ought to be proportional to those indicated by *permanent* deflections: that is, when steady forces shown by permanent deflections from different sources are equal, the initial increments of those forces ought to be equal also; or if the steady forces be unequal, still the initial incremental forces shown by transient deflections ought to be proportional to them. So that if Δ and Δ' denote the angles of steady or *permanent* deflections from two different sources of excitation, and δ and δ' the corresponding angles of *transient* deflection, produced by sudden discharges, each acting for some small standard interval of time, from the same sources respectively; then, by taking our former values of the electric forces in terms of the angles of deflections, we ought to have

$$\frac{\operatorname{Tan.} \Delta'}{\operatorname{Tan.} \Delta} = \frac{\sin. \frac{1}{2}\delta'}{\sin. \frac{1}{2}\delta} : \quad . \quad . \quad . \quad (c)$$

And when $\Delta = \Delta'$, so should $\delta = \delta'$.

But, in electric discharges generally, it is a well known fact that the initial forces are superior to those subsequently exhibited in any similar interval of time, provided no greater a degree of excitation be carried on during the discharge than at or before its commencement. This fact is particularly applicable to the discharge of a Leyden jar, the *force* of which depends upon the elasticity of the fluid superinduced on its surface. The elasticity in this case is proportional to the *density* of the fluid on the

* We are very well aware that a magnetic needle may be kept perfectly steady by a rapid succession of impulses of equal intensity, which could not be strictly called an "equable flow" of the fluid; but, even in that case, equal quantities would be transmitted in equal intervals of time.

coating; and as the density is in its most exalted state at the commencement of the discharge, it follows that the *initial* force is superior to the force taken at any subsequent period of the fluid's motion; and, in consequence of the progressive diminution of density in the jar, the force in the circuit necessarily lessens proportionally, from the first to the last moment of transmission.

The *velocity* of the fluid in the circuit is also dependent on its elasticity in the jar. Hence it is that the initial velocity is a maximum in every discharge from a jar; and, for the same reason, the *quantity* transmitted in the first interval of time is .greater than in any other similar interval during the discharge. This is universally the case, whatever may be the size of the jar, or with whatever quantity of fluid it may be charged.

It is obvious also, that the initial force from any, the same standard quantity of fluid, will vary with every variation in the extent of the coated surface over which it is distributed; for the initial force in the circuit depends upon the elasticity of the charge, and the elasticity upon the density. But the density varies in the inverse ratio of the extent of coated surface over which the fluid is distributed; hence the initial force of the same quantity varies with every variation in the extent of coated surface.

These circumstances are inseparably connected with the discharge of electric jars, whatever be the nature of the conductor through which the fluid is transmitted, provided its conduction suffers no change during the process. But the *effects* absolutely produced, and the time of transmission, will be modified considerably by the character of the conductor.

The most protracted discharge of a Leyden jar, however, is too transient to command even a moment's steady deflection of a magnetic needle. On this account, therefore, as well as in consequence of an unequable flow of the fluid during the transmission, *permanent* deflections are absolutely unattainable, and consequently the admeasurement of electric forces by those means entirely out of the question; and the action of a current directly from the machine has never yet been shown to afford the necessary data.

Thus circumstanced, we are necessarily compelled to abandon all hopes of ever arriving at experimental results, from the electrical machine, that could at all favour the idea of lessening our future labours by electro-dynamic ratios of permanent and transient action. Notwithstanding, however, the impracticability of obtaining *permanent* deflections from the discharge of a jar, or from currents direct from the machine, they are very easily accomplished by availing ourselves of other sources of electric action, which, fortunately for inquiries of this kind, are both abundant and of easy access. It may, therefore, not be amiss in this part of the discussion, to make ourselves acquainted with a few experimental results from various sources, which are at immediate command. And in order to obviate any obscurity which might happen to occur from a want of uniformity in the arrangement of the experi-

ments, we have been led to a selection of a standard angle of five degrees for the permanent deflections, from whatever source they may proceed: these are all arranged in one vertical column in the following table; the transient deflections are placed in the same lines in a column on the right, and the respective sources of electric action are seen on the left. One second of time was allowed for the electro-dynamic action to produce the transient deflections.

The Galvanometer employed in these experiments has a rectangular coil, each side of which is six inches, and consists of eight convolutions. The needle, which is four inches long, and furnished with an agate cap, is placed in the centre of the vertical coil on a fine steel pivot; and a thread of sewing silk, with a brass ball at its lower end, constituted the second's pendulum. The metals employed in the Voltaic experiments were a strip of copper and another of zinc, each one inch broad, and placed about a quarter of an inch apart. The thermo-electric apparatus was a thin bar of bismuth with a copper wire at each end.

Table of Permanent and Transient Deflections of a Magnetic Needle by Electric currents from various sources of excitation.

Source of Electric Action.		Deflections.	
Copper and zinc area of surfaces immersed.	Exciting Liquid.	Permanent.	Transient.
Square Inches.			
4 . 0	Water and Sulphuric acid . . .	5°	15°
6 . 0	Weaker acid solution	5°	12°
0 . 6	Stronger solution than first . . .	5°	10°
4 . 0	Water and Muriatic acid . . .	5°	13°
0 . 6	Stronger acid solution	5°	10°
4 . 0	Water and nitrous acid	5°	10°
0 . 2	Stronger solution	5°	11°
6 . 0	Common salt and water with a few drops of nitrous acid . . .	5°	47°
Bar of bismuth and copper wires.	Excited by a Spirit Lamp.	5°	9°

The tabular transient deflections exhibited in the third column, although they do not express a close correspondence among themselves, appear to have an approximate relation to the permanent standard deflection, which might perhaps be more fully established by means of extensive series of experiments. The mean of all the first seven deflections would be $11\frac{7}{11}°$; and the permanent to the mean transient as 1 : 2·346 nearly. But if we compare Experiments 1, 4, and 6, in which the same extent of metallic surface was employed, a considerable discrepancy is immediately seen—the extremes differing five degrees, or half the quantity of the lesser deflection; so that, as far as regards the experiments in which acid solutions were employed, very little in common is perceived amongst their results. There is, however, in these first seven experiments one striking feature deserving of notice, which is, that the angles of transient deflection are not less than twice that of permanent deflection. The ninth, or thermo-electric experiment, also appears to approach the same law. Other Galvanometers, and other intervals of time for the action of the transient force, have, however, given very different results. The eighth experiment, in which salt and water were employed, exhibits a very different law to any of the rest, the transient deflection indicating an electro-dynamic action of about three times the intensity of that of the greatest of those with acid solutions only.

Hitherto the formula, c, has been considered as applicable to those cases only in which the electro-dynamic action is supposed to be constant from the first closing of the circuit; but it would be equally applicable whether the initial and subsequent actions were uniform or not, provided they exhibited a constant ratio in all the individual currents under contemplation: for if F' and F represent the permanent forces of two currents, and f' and f their respective initial forces, then since

$$F' : F :: f' : f, \qquad \frac{F'}{F} = \frac{f'}{f}$$

And by substituting the respective values of those forces, in terms of the arcs of deflection, we have

$$\frac{\text{Tan. } \Delta'}{\text{Tan. } \Delta} = \frac{\sin. \frac{1}{2}\delta'}{\sin. \frac{1}{2}\delta} \quad \text{the same as before.}$$

There are only three cases, however, in the tabular results in which this ratio is exhibited—viz. the third, fifth, and sixth: in those it is shown that the permanent is to the transient as 5 : 10, or as 1 : 2. All the rest exhibit different ratios, no two of which are alike; and although our experimental inquiries have been pursued to a considerable extent, and conducted with great care, employing different intervals of

time, from half a second to three seconds, for the play of the initial force, the prospects they have unfolded towards arriving at the analogy sought for are no more promising than in those exhibited in the table.

With regard to Voltaic arrangements, the action on the needle varies with almost every circumstance which enters the process of experiment; and the exact* ratios of permanent and transient forces which they develop are probably as exhaustless as the variety of particulars, both intentional and incidental, that happen to be connected with the process. The character of the exciting liquid—the character and extent of the metallic surfaces—their state of oxidation—their distance from one another—the time the circuit has been broken—are amongst the most obvious of the probable multitude of circumstances which influence the initial electro-dynamic action.

The sources of electric action which appear the most favourable for developing uniform results are those in which *heat* is the exciting medium. The circumstances connected with this process being very limited, the experiments are not so liable to be influenced by casualties as those in which liquids are employed.

The difference of opinion which prevails among scientific men regarding the indications of the Galvanometer, as a measurer of the "absolute quantity" of electric fluid developed by various processes of excitation, may possibly arise from a want of due attention to the *modus operandi* of transmission. Perhaps a few hints may tend to develop other views of this subject than those which have hitherto been taken, and to reconcile the apparent discrepancies with which the phenomena are attended They may very properly be prefaced by an

Experiment.—A pair of copper and zinc cylinders, the former exposing about sixty, the latter about twenty square inches of surface to a strong solution of salt and water, had been in electrical play for forty-six hours, at which time the permanent deflection was reduced to 2°. The circuit was now broken (without disturbing the metals) for ten minutes, then closed for one second of time: the needle was driven through an arc of 75°.

It would be exceedingly difficult to make it appear that this Voltaic pair, or indeed any other of moderate size, could hold in suspense a charge of the electric fluid greater than that which could be given to an extensive surface of glass, by the common machine; yet, perhaps it would be as difficult to show that the transmission of an intense charge of fifty square feet of coated surface would produce a similar effect on the same Galvanometer. Nothing of the kind has hitherto been done; neither are we aware that any experiments, or any train of reasoning have yet been attempted, which would satisfactorily explain the obvious difference of the Galvanometer

* We say "exact ratios," because, when very light needles are employed, perhaps no two would be found precisely alike. Those results which are placed in the table were arrived at by using a comparatively heavy needle.

indications exhibited by discharges from the machine, and those from other sources of electric action.

The " absolute quantity" of fluid put into motion by a Voltaic pair may possibly be very small, when compared to that constituting the charge of a Leyden jar of even moderate dimensions ; and yet its electro-dynamic action on the needle might be much greater than that of a discharge from the jar ; because, in the former case, the fluid would circulate a multitude of times in a comparatively compact body, whilst in the latter case (the same time being allowed) the charge would be drawn out, as it were, into a thin slender stream, made up of an infinite number of small parts of the " absolute quantity," or whole charge.

In the one case the effect due to a certain quantity of fluid is reproduced with infinite celerity, which operates on the needle as a series of distinct discharges, succeeding one another with sufficient rapidity to produce a deflection which would indicate a force equal to the sum total of the series, or equal to the momentum arising from the number of circumvolutions performed by the fluid in a given interval of time. If, then, the effect due to one transit of the " absolute quantity" be represented by m, and the number of repetitions in the series by n, the momentum on the needle in the first interval of time would be $n\,m$. If we suppose the number of repetitions of the unit of force, in one second, to be $1{,}000 = n$, then, whatever be the value of m, we should have the momentum, or whole electro-dynamic action on the needle, in one second, equal to $1{,}000\,m$; or the effect on the needle would be that of a thousand single discharges of the standard or " absolute quantity" of fluid ; but if the same " absolute quantity" be discharged from a jar, and it requires one second of time for its transmission through the Galvanometer coil, we can have no more electro-dynamic action than that produced by the sum total of its parts (whether they be equal or unequal) once produced, or by one solitary transmission of the " absolute quantity" from the positive to the negative surface.

If we be permitted to conduct our reasoning in this manner, we shall obviously be led to understand that the " absolute quantity" of fluid in motion, separately considered, may possibly perform but a very insignificant part in the production of magnetic deflections, and must at all times be but very imperfectly represented by the Galvanometer.

The inquiry, however, hitherto pursued, relates only to the deflections of a magnetic needle as admeasurements of the electro-magnetic action of the conducting wire of the coil, simply considered as a *deflecting force*, without any regard whatever as to the *nature* of that force ; and independently of the character of the elements of which it is composed, or by what means it is produced. There are yet, however, several interesting parts of the subject which demand attention, and which we mean to consider at some future time; but the facts already stated in this Memoir will

probably appear sufficiently influential to deter philosophers from placing implicit confidence in the indications of the Galvanometer, irrespective of their distinctive characters, and of the circumstances connected with the source of electric action.*

W. S.

Woolwich, September, 1836.

ON MARINE LIGHTNING CONDUCTORS. †

TWENTY-FIRST MEMOIR.

1. In addressing this Memoir to the British Association for the Promotion of Science, I am far from supposing that there can exist a necessity for entering into a routine of details, in order that the subject may appear sufficiently important for its consideration. It is one well known to be at all times interesting to the philosopher; and at the present moment, when the British fleets are about to be furnished with lightning conductors, it has become a subject of high national importance, and demands the most profound consideration of every experienced Electrician in the land, in order that the most efficient and economical plan may be adopted and carried into effect.

2. Without any pretensions to a knowledge of the motives for not making so momentous and unsettled a topic open to fair scientific discussions, there can be no doubt of the scantiness of publicity being the sole cause of so *few plans*‡ being brought before the Committee appointed to inquire into the best means of protecting shipping from the effects of lightning. I am not aware of the reasons for giving a preference to that so long proposed by Mr. W. S. Harris; but as the House of Commons has been officially informed that the Committee have determined in favour of the con-

* In a highly interesting Memoir, descriptive of a course of experiments on Magnetic Electricity, by Professor Christie, the law of *transient* deflections was strictly observed. (*Philosophical Transactions*, for 1833.) This, I believe, is the only instance on record.

† Addressed to the British Association for the Promotion of Science. Birmingham meeting, September 8th, 1839.

‡ Only two plans were brought before the Committee, the one by Mr. Martin Roberts and the other by Mr. W. S. Harris. The plan of the former gentleman, which has long been employed in the French navy, consists of a rope of metallic wire, one end of which is hoisted to the mast-head, and the other thrown over the side of the vessel. (See *Annals of Electricity, &c.* vol. i, page 468, vol. ii, page 241.) The plan of the latter gentleman was originally proposed by the late Mr. Henley, in the the year 1774, and is minutely described in the *Philosophical Transactions*. It consists of strips of sheet copper, one to each mast, let into a groove the whole length of the after side, and passing through the keel of the vessel; that belonging to the mizen-mast passes either through or close to the after powder magazine.

ductors proposed by that gentleman, the subject is now open to impartial discussion; and as it is possible that, unless some timely council interpose, the recommendations of the Committee may induce the Admiralty to adopt those conductors, and without further inquiry respecting the efficiency, or inefficiency, in parrying the effects of lightning, give immediate orders for their application to Her Majesty's fleets, no time ought to be lost in placing them in a *proper light* before the Government and maritime community of these realms.

3. There certainly cannot be a finer opportunity offered to the British Association for exercising its almost boundless influence in eliciting important philosophical truths, and extending the benefits of experimental and practical science, than that now presented by this momentous topic ; nor can Electricians of every country have a more favourable opportunity of becoming benefactors to mankind, than by giving this universally important subject their profoundest contemplation, and as soon as possible making known the results of their experimental investigation in this branch of Electricity, and their uncompromising opinions on a system of conductors on which so much deep interest is now involved, or by proposing any better plan of protection to the thousand of lives and millions of property continually exposed to the most formidable and distructive of nature's elements.

4. The present Memoir contains—*First*, an examination of those experiments which Mr. Harris has exhibited on various occasions, and at various places, in illustration of the supposed superiority of his system of conductors. *Second*, an examination of the observed effects produced on shipping by lightning. *Third*, a statement of electrical phenomena similar to those effects, and probably productive of others no less destructive. *Fourth*, a comparison of the observed effects of lightning, and the probable effects which lightning would produce by the adoption of Mr. Harris's proposed system of conductors. *Fifth*, a description of a new system of conductors for the protection of shipping from lightning.

Examination of Mr. W. Snow Harris's Experiments.

5. *Experiment* 1.—Let *j*, Fig. 1, Plate XIV. represent the electrical jar on board the Caledonia, C the Louisa cutter, with a strip of sheet copper let into the after side of her mast from top to bottom, and continued through her keelson to the sea, as represented by the strong black line. By means of a chain, *o o o*, the lower extremity of this conductor is brought into metallic connection with a loaded brass howitzer in the boat, B, moored at some distance from the cutter. A wire, *w w w*, proceeds from the outside of the jar to the boat, and terminates above the vent of the howitzer, leaving a small interruption for the introduction of a priming of *detonating* powder. By means

of another wire, *w' w'*, one end of which is fastened to the conductor at the mast-head of the cutter, the jar is discharged in the direction indicated by the arrows; and the spark which passes between the howitzer and the end of the wire, *w w w*, ignites the *detonating* powder, and the piece thus becomes discharged—whilst a portion of *common gunpowder*, placed in contact with the *uninterrupted* conductor at *p* in the cutter, *is not exploded.*

Experiment 2.—The only difference in this arrangement from that in the last experiment, is simply that of interrupting the metallic connection between the lower extremity of the cutter's conductor and the howitzer in the boat, B, and permitting the sea to form part of the circuit. The effects were the same as in the first experiment.

Experiment 3.—The arrangement of apparatus the same as before, with the exception of an interruption in the cutter's conductor at *p* by passing a saw through it. No gunpowder nor *detonating* powder at *p*. The howitzer was discharged as in the preceding experiments.

6. These three experiments were made in Plymouth harbour, in September, 1822, "in presence of the Navy Board, Sir Alexander Cochrane, Commissioner Schield, several Captains of the Navy, and principal Officers of the Dockyard," with the intention of convincing them of the efficacy and superiority of Mr. Harris's proposed conductors; although it must be obvious to every Electrician that these experiments had no bearing on any peculiar plan of conductors whatever, for they are just as applicable to the illustration of the efficacy of the *present chain conductors* or to the *metallic rope* proposed by Mr. Roberts, as to those proposed by Mr. Harris.

7. The first experiment showed that copper is a conductor of Electricity; that *detonating* powder can be ignited by an electric spark; and that *common gunpowder* will *not* ignite by the application of cold copper, though a feeble momentary electric current traverses the metal at the same time. The second experiment, in addition to the facts shown by the first, shows that sea water is a conductor of Electricity. The third experiment, in addition to the facts shown by the first and second, shows that a *slight* interruption in the metallic part of the circuit did not prevent a spark from the jar employed, from igniting *detonating* powder at another *slight* interruption of that circuit, at the vent of the howitzer.

8. These, I believe, are the only facts which Mr. Harris's experiments, exhibited in presence of the Navy Board, are calculated to illustrate; and, as they prove nothing novel, nor anything peculiar to that gentleman's proposed system of conductors, they not only fall short of their object, but are perfectly inapplicable to the great question at issue.

9. There are, however, circumstances connected with these experiments, which, though not noticed by Mr. Harris, would have been quite as interesting to the Navy

Board as the results which they witnessed. Mr. Harris could easily have shown the officers of the Navy Board that a moderate electrical discharge would render a copper conductor *red hot*, and that gunpowder in contact with this electro-heated metal would explode as decidedly as if heated by any other means; and it would have been no more than a just inference to have added that a lightning conductor may be sufficiently heated to fire gunpowder by a discharge of lightning from the clouds. It would have been well also to have shown that *detonating* powder would be as sure to ignite by the spark at the interruption made by the saw in the cutter's conductor, as by a spark at the howitzer. From the first of these facts, those officers might probably have inferred that a lightning conductor, passing through the powder magazine, would be the most dangerous appendage in the ship; and from the latter they would have implied that the most trifling interruption in the conductor, from accident or otherwise, might be productive of serious evils.*

10. In a lecture delivered before the members of the British Association, at the Liverpool Meeting, Mr. Harris brought forward the following additional experiment, in illustration of the advantages of his system of lightning conductors.

Experiment 4.—In Fig. 2, *a b* and *c d* represent the two halves of a long tapering round pole, cut along its axis from end to end, having the flat sides of both halves towards the spectator. In the axis of *a b* are imbedded three pieces of metallic wire, *a n*, *n m*, and *m b*. At the interruptions, *n* and *m*, are small cavities in the wood for the lodgment of gunpowder and *detonating* powder; and, when charged with these combustibles, the flat side of *c d* is placed against the flat side of *a b*, the two being slightly kept together by a suitable contrivance. The outer surface of this compound piece is furnished with a strip of tinfoil, which reaches from top to bottom; and is then represented as a perfect model of a ship's mast, furnished with Mr. Harris's conductors.

When a jar is discharged from the top to the bottom of this mast, the electric fluid traverses the tinfoil on the outside, and the mast is protected. This done, a portion of the tinfoil conductor, reaching from above the opening at *n* down to below the other opening at *m*, is taken away; and the next discharge of the jar necessarily passes through the axial wires, and ignites the powder in the two cavities, *n* and *m*; the explosive force of the gunpowder blows off the moveable half, *c d*, of the mast. This effect was held forth as an exemplification of the effects of lightning on a ship's mast.

11. Whatever may be the merits or demerits of Mr. Harris's system of conductors, it is very remarkable that the experiments exhibited by that gentleman in its favour,

* I believe, however, that Mr. Harris has taken every precaution to prevent interruptions in the conductors which are applied to the masts; but at the steps of the masts, and between the copper bands and bolts in the keelson, it will always be difficult to keep metallic contact so as to prevent explosions.

N.B.—Since this Memoir was originally published, two instances have occurred in which lightning separated the copper plates from one another. See note to 26 27.

before the Navy Board, at Plymouth, in 1822, and repeated in his lectures before the British Association, at Liverpool and other places, happen to have no bearing whatever on the subject. Those experiments are no more illustrative of the efficacy of Mr. Harris's system than of any other system ever yet offered to public notice; and, as I am not aware that any other experiments have been attempted for demonstration, I have no means of knowing how that gentleman has been led to neglect some of those considerations, which, next to the selection of the conducting material, are certainly the most important in the equipment of shipping with lightning conductors.

EXAMINATION OF THE OBSERVED EFFECTS PRODUCED ON SHIPPING BY LIGHTNING.

12. A general outline of the effects of lightning on shipping cannot be better pourtrayed than by the following description of those produced on the *Rodney*, on the 7th of December last:—

Fatal Effects of Lightning on H. M. S. Rodney, in the Mediterranean.

H. M. S. *Rodney*, 7th December, 1838, was eight or ten leagues to the eastward of Cape Passaro, blowing strong, with squally, overcast, and rainy weather, under close-reefed fore and mizen and treeble-reefed main-topsail. At nine, A.M., in a heavy squall, attended with hail and hard rain, the ship was struck with lightning. Three or four flashes of angry-looking forked lightning had been seen before in the northern horizon, not attaining an altitude of apparently more than 10 or 15 degrees; and the writer thinks it here necessary to mention that this statement is drawn up entirely from his own ideas, sensations, and observations on the occasion, and not from any hearsay reports, because many on board saw and heard things differently to himself. He was standing with Captain Parker close to the wheel at the time. No very particularly vivid flash or unusually great glare of light was observed at the time, although people are commonly blinded for some seconds on such occasions; but the first thing heard was the burst or explosion, which was louder than one of the ship's thirty-two pounders, but not exactly like it, it being sharper and more piercing, something like the bursting of a bomb-shell; and then the iron hoops of the main-mast came rattling down the mast, being rent asunder. The Captain and the writer looked at each other in silent astonishment, then forward and aloft, when the hoops were seen tumbling down the mainmast, and splinters flying about in all directions, and to leeward, to an immense extent. The main-topgallant-mast

was shivered to atoms, leaving the main topgallant-yard across the cap, and the royal pole from the hounds of the topgallant-rigging to the truck, perfect, which was left sticking up above the topgallant-yard, having slipped itself on the upper part of the topmast-rigging. In perhaps about a minute or so, flames were seen issuing from the bunt of the topsail-yard, the electric fluid having ignited the paunch mat and gear thereabouts; it burnt some minutes before it could be extinguished by wet swabs, water, &c. Fortunately, from the heavy rain, everything was perfectly saturated, or no one knows where the mischief would have ended. The electric fluid was seen to pass from the mast about seven feet above the deck, over the starboard hammock netting, to leeward, right over a gun, like a ball of fire. Luckily all the people on deck had been ordered to take shelter out of the rain, consequently nobody was near the mainmast at the time, and nobody suffered excepting the unfortunate men who were at the mast-head.

Progressive Course of the Electric Fluid.—The vane staff, which is six feet long, with a copper spindle (on which the vane traverses) of about ten inches in length, surmounted by a gilt wooden ball, the size of an orange, shows its first effect—(the ball and spindle were never seen after the shock)—being split, but not broken, and one side of it blackened. The copper binder round the truck was burst asunder, a small piece broken out of the truck, and one of the metal sheaves for signal halyards slightly fused. From this, after leaving the royal pole uninjured, it appears to have passed inside the copper funnel for topgallant-rigging and iron hoop of the hounds of the mast, shivering the topgallant-mast to atoms; from thence to the topmast-cap, not a piece having been seen the size of a common walking cane, and the sea was literally covered with its splinters to a considerable extent; its marks are now lost for many feet, notwithstanding the shock about this spot must have been most terrific, as it was in the topmast cross-trees where the poor fellows who suffered were at the time; and also the heel of the topgallant-mast (which was not at all injured below the cap) was forced upwards into the cap, the fid being raised about eight inches above the trussel-trees with such force that the top burton-block strop was carried away in trying to bouse it down again; and, after all, we were obliged to cut it out, not being able to clear it in any other way. Its next appearance is on the main-top-mast, ten feet above the cap, seemingly attracted by the iron-bound tye-blocks and iron hoops on the topsail-yard (being under a treble-reefed topsail), from whence it rent an immense splinter out of the mast down to the lower cap, going nearly into the core of the mast, and set fire to the tarry and greasy gear about the bunt of the topsail-yard, after taking this large splinter of nearly one-quarter of the substance of the mast away. Its next positive mark is on the starboard lower trussel-tree; the lower cap, head of lower-mast, and heel of topmast (both iron hooped) having escaped unhurt. It shook and blackened the trussel-tree, rendering it unserviceable,

and then must have entered the mainmast, spreading and passing down both sides, bursting thirteen of the large iron hoops in its course, and knocking out pieces of the side-trees and main-stick in several places, and escaped from the mast in the shape of a fire-ball, seven feet above the deck; and was seen to go over the starboard (leeward) netting, right over the gun abreast of the mainmast, rending the hammock cloth in several places, carrying away one rattlin and stranding another. Its exit, although fiery in appearance, was harmless in effect, merely injuring the cloth over a space of about a foot, and breaking the two rattlins, when it was seen to strike the water a short distance from the ship.

Effects of the Electric Fluid in its Course.—Knocked overboard (at least they were never picked up or seen) the gilt ball, copper spindle, and calico vane from the top of the vane staff—split the vane staff—broke the copper binder round the truck—broke a piece out of truck and slightly fused one of the metal sheaves for halyards—cleared away the whole of the main topgallant-mast from the hounds of topgallant-rigging to the topmast-cap, not leaving a fragment aloft. Four men, who had been sent aloft to unbend topgallant-gear and prepare for sending the yard down, were in the cross-trees at the time. John Rowe was struck dead as he was moving from the weather to the lee side of the mast for shelter from the rain; he was just on the aft side of the mast at the moment, and fell astride the after cross-tree, where he was held by some ropes falling round him: he never spoke. Thomas Hollingsworth was standing on the after shroud of topgallant-rigging, to leeward of the mast, and holding on by the after cross-tree: he was so seriously injured as to be sent down in a chair and died in seven hours after. Hugh Wilson was standing on the foremost shroud of topgallant-rigging, holding on by foremost cross-tree and close to Hollingsworth. He states that the shock threw him forward and Hollingsworth aft: he was only slightly hurt, and only two or three days in the doctor's list. The other man, Charles Prynn, was to windward, standing on the cross-tree, holding on by the foremost shroud of topgallant-rigging, and received so slight a shock that he did not even apply to the doctor. Wilson heard no thunder. The first named two men had every stitch of clothes burnt from their bodies, excepting just the wrist bands and lower parts of the trousers which was left about the wrists and ancles. They presented a shocking spectacle, their bodies discoloured and hair singed from their persons. The next place is a large splinter out of the main-topmast, from ten feet above down to the cap, setting fire to the gear about the topsail-yard, and then commences its destructive force about the mainmast, first of all giving a severe shake to the starboard lower trussel-tree. It is hardly possible to give a description of its effects on the mainmast—the mast should be seen fully to understand it; but some idea may be formed when it is stated, that out of twenty-eight large iron hoops, five inches wide and half-an-inch thick, between the deck and trussel-trees, thirteen were burst assunder; and that for a space of fifty-three

feet is ravaging effects can be traced the whole way, and the spot whence it made its final escape is several inches deep in the mast—on the starboard side a large piece of the mast is broken out (six inches deep) from the third to the sixth hoop above the deck, and from the eighth to the ninth hoops. The cheek or side-tree, several feet of the lower part gone altogether, and the other part nearly shook all to pieces. The larboard side—ekin piece gone from the sixth to the eleventh hoop, and the mast burst out from the ninth to the eleventh, and from the thirteenth to the fifteenth, and the cheek very much shook. The hoops carried away were mostly the clasp hoops of side trees, but some of the body hoops were also burst asunder; and, strange, to say the awning hoop, on which the main-trysail-mast, steps, and mizen-stays reeve, lost one of its fore-locks, notwithstanding a piece of copper had been nailed over the clasp part: the fore-lock, which was driven downwards, was gone, with a piece of copper, and never seen, while the one which drives upwards was left in its place, and held the hoop together. There were eighteen body hoops between the deck and trussel-trees, and ten clasp hoops round side-trees: four of the body hoops below side-trees were broken; none of the hoops on the head of lower-mast, or on the heel of the topmast, were touched.

Several men assert that balls of fire were running about the lower deck, and that they ran after them to throw them overboard. This seems strange, but if so, and it is hardly possible several could be deceived, it could be nothing more than flashes or rather sparks passing down the different hatchways after the explosion, and less active than in the first descent: at all events, it is certain there was a strong sulphurous smell below, particularly in the pump well, and sparks seen by many of the officers. It is remarkable that the electric fluid seems to have jumped from metal to metal, first the copper spindle, then the copper funnel of topgallant-rigging and iron hoop round the mast, to the head of the topmast; from thence to the iron-bound blocks and hoops on the topsail-yard to the main-cap; and thence to the lower trussel-trees, taking all the hoops downwards, passing over a gun into the sea.

The mast has since been taken to pieces at the naval yard at Malta, and its interior shows no defect. In fact not the slightest injury appears about the mast, except what was exteriorly displayed; it is marked in some places, even on the spindle (centre-piece), as if a train of powder had been flashed on it but nothing more.

The outside crippled state of the mast led us to fear the worst, and to secure it as effectually as possibly for carrying sail. This was done by getting the spare fore-topmast up and down, filling up with studding sail booms, and clapping the iron fishes over the worst part—the main-topmast being lowered sufficiently to bring its wound below the cap. All was well woulded together, and by degrees we felt confidence in the strength of the mast, so that at last we got a fore-topgallant-mast fitted on it, and carried the mainsail, treble-reefed topsail, topgallant-sail, and royal, working to windward for three or four days. G. B. H.

Magnetism and Electricity.—(Extract of a letter from Malta.)—After the mainmast of her H. M. S. *Rodney* was struck by lightning, during her passage from Athens to this place, the broken hoops surrounding it were all found to be magnetized in the same uniformity of direction as if they had been operated on in one direction by the Galvanic helix. Thus in a hoop broken in two athwart ships (speaking with reference to the ship's head), the larboard end of the foremost portion was a south and its starboard end a north pole: the end of the aftermost portion in contact with the south pole of the foremost portion, being consequently a north pole, and the other end thereof a south, and so uniformly with all the other hoops at whatever part they were ; similar poles in each hoop always pointing in similar directions in the circumference of the respective circles.—*Nautical Magazine.*

13. There are five remarkable facts stated in the above account from the *Rodney*, which will be occasionally alluded to in this discusion—viz. the bursting of the metallic hoops of the mainmast, copper binder of the truck, &c. ; the fusion of the metallic sheave ; Wilson and Hollingsworth being thrown in opposite directions ; the flashes or sparks seen below deck ; and the manner in which the broken hoops were magnetized.

14. The following are cases in which ships, furnished with Mr. Harris's conductors are supposed to be struck with lightning.

Extract from a Report on the Lightning Conductors of H.M.S. Beagle. 1831-6.

" Previous to leaving England, in 1831, the *Beagle* was fitted with the permanent lightning conductors, invented by Mr. W. Snow Harris, F.R.S.

" During the five years occupied in her voyage, she was frequently exposed to lightning, but never received the slightest damage, although supposed to have been struck on at least two occasions. At each of these times, at the instant of a vivid flash of lightning, accompanied by a crashing peal of thunder, a hissing sound was heard distinctly on the masts, and a strange though very slightly tremulous motion in the ship herself, indicated that something unusual had happened, &c.

(Signed) " ROBERT FITZROY,
" *Late Captain of H.M.S. Beagle.*"

Extract from a Copy of a Letter from Captain Turner, late of H.M.S. Dryad.

" H.M.S. *Dryad*, Sierra Leon, Feb. 13th, 1831.

" MY DEAR SIR,—I write to inform you that we have had a trial of your conductors and most excellent things they are. During our last cruise we had a great deal of lightning, but in one afternoon in particular we had a tornado, which is always attended with a great deal of thunder and lightning. Whilst standing on the quarter-deck during one of the flashes, I distinctly saw the lightning run down the con-

ductor on the foremast; and the officer of the forecastle came and told me he heard a whizzing noise resembling water boiling: all the men that were there heard it also. A short time afterwards several of the officers were standing abaft, and saw it, during another flash, go down the mizen-mast with the same whizzing noise. It may be necessary to tell you that lightning on this coast appears to remain longer in the air than any other place I was ever at.—I am, &c. &c.

"WILLIAM TURNER."

15. Mr. Harris observes, that there is "a remarkable agreement" in these two cases, especially in "the hissing sound, &c." On this phenomenon I shall remark, that the hissing sound proceeds from no other parts of a conductor than those which deliver the fluid into the air or other inferior conducting medium; for no such noise is ever produced by the fluid *entering* a metallic conductor. Hence it is obvious that if these hissing sounds proceeded from Mr. Harris's conductors, they were occasioned by the electric fluid rushing from the sharp edges of them, and from numberless asperities on the surface of the metal. To those accustomed to making electric-kite experiments during thunder storms, the hissing noise must be a familiar phenomenon. It proceeds from the broken parts of the metallic strand and other asperities of the string, which are discharging fluid from the *upper* to *lower* strata of air, or to the ground, and not from any flash of lightning striking either the kite or its string. Hence, the hissing noise heard on board the *Beagle* and *Dryad* was no indication of either of the vessels receiving a flash of lightning at the time; but rather that their conductors, and other parts of the rigging, were discharging into the air, about the *lower* part of the masts, a quantity of fluid communicated to their *upper* parts by a *wave* of the electric fluid produced by a neighbouring flash of lightning. There is certainly something remarkable in the appearance of lightning running down the masts of the *Dryad*, which I shall endeavour to explain under another head; but whether they emanated from direct discharges of lightning on the masts or otherwise, they could be no very welcome visitors in the powder magazine.

16. Mr. Harris has stated, that "from about 100 cases, the particulars of which have been ascertained, it appears that about one-half of the ships struck by lightning are struck in the mainmast; one-quarter on the foremast; one-twentieth on the mizen-mast, and not more than one in a hundred on the bowsprit. About one ship in six is set on fire in some part of the masts, sails, or rigging. In these 100 cases there are destroyed or damaged 93 lower-masts (principally line-of-battle ships and frigates), 83 topmasts, and 60 topgallant-masts."*

17. By looking over the particulars of 174 cases,† which Mr. Harris has collected, I find only 44 in which the topgallant-masts appear to have been injured; and as out of

* Harris's "State of the question relating to the protection of the British Navy from lightning."

† In several of these cases the particulars of the damage is not specified.

these 44 cases there are 13 in which the topgallant-masts were lost, broken or damaged—accidents probably occasioned by the mere falling of those masts when the others below them were struck—there would appear to be only about 31 cases out of the 174 in which the topgallant-masts have been absolutely struck by the lightning. It is probable, indeed, that the proportion is even less than this; because of these 31 cases, those occurring near to the heel of the masts might easily arise from lightning striking the ship no higher than the topmast-head. Lightning striking the topsail yard-arm, when that sail is set, or the cross-trees at other times, would be very likely to damage the lower part of the topgallant-mast.

18. An accurate statement of the *highest point* which lightning has struck each vessel, and the cases in which yard-arms have been struck, would afford important data for showing the necessary height to which conductors ought to ascend in the rigging, and the comparatively uselessness of continuing them higher: also how far it would be advisable to protect the yard-arms for the purpose of preventing lightning entering the vessel by their instrumentality.

ELECTRICAL PHENOMENA SIMILAR TO THOSE PRODUCED BY LIGHTNING.

19. The figure, dimensions, and character of the metal being determined on, the next most important consideration, in the application of lightning conductors to shipping, are their most eligible situations, both as respects the working of the sails and avoiding the probable disasters by lightning entering the rigging and hull of the vessel; and to determine which, it becomes necessary to study the effect of both the *direct* or main stroke and the *lateral* forces to which it gives rise.

20. By the *direct* force or main stroke we know that men can be killed, inflammable bodies ignited, and metals even fused. Hence our first object should be to keep the *direct* discharge as clear of the ship as possible, especially from the masts, deck, and hull; and as it is possible that the most spacious conductors that can conveniently be applied to a ship may be rendered sufficiently hot by a flash of lightning to ignite gunpowder,† we ought never to be induced, under any circumstances,

† Instance the fusion of the metal sheave of a block on board the *Rodney*. Some few years ago the Brig *Jane*, from New York to Liverpool, had some part of her *chain conductor* fused by a flash of lightning; the lower part of it fell overboard. It is said that the lightning rod, passing through the Nelson monument, at Edinburgh, became so hot, by a flash of lightning, that it could not be touched by the hand by the first person who visited it afterwards. Allowing only a few minutes to have elapsed between the flash and the person entering the monument, the probability would be that the conductor had been rendered red hot by the lightning. The lightning which struck St. Bride's Church, London, in the year 1764, "bent and broke assunder an iron bar, 2½ inches broad, and ½ inch thick;" and in the year 1759, in the Island of Martinico, a flash of lightning nearly destroyed by fusion a bar of iron, about 4 feet long, and 1¼ inch thick. (*Philosophical Transactions*, 1764.) In the year 1772, St. Paul's Church, London, was struck by lightning, which heated to redness a portion of one of its conductors, consisting of a bar of iron nearly four inches broad, and about ½ inch thick. (*Philosophical Transactions*, 1772.)

whatever, to lead a lightning conductor through or near to the powder magazine, at the risk of blowing up the ship.

21. The *lateral forces* are of three distinct kinds, which, for convenience of reference, I will denote by the *first*, *second*, and *third* kind, as they are known by their respective phenomena.

22. The *first* kind of lateral force was, by Priestly, called the *lateral explosion.* It takes place at every interruption of a metallic circuit, or wherever the electric fluid is exhibited in the shape of a spark or sudden flash of light in the common atmosphere, or other inferior conducting medium. By this kind of lateral force, the air is suddenly displaced, loose bodies are scattered and thrown from the axis of the circuit, and solid inferior conducting bodies are shattered or torn to pieces; *waves* of Electricity are also produced in the neighbouring atmosphere, and the bodies within it, by this kind of lateral force. Hence, the probability of ships' masts and other masses of wood being shivered, split, &c., their hoops burst asunder, and other similar effects being produced from the same cause: even men may be killed by these lateral forces (12-13).*

23. The *second* kind of lateral force is known by a species of radiation of the electric matter from the surface of good conductors, carrying a *direct* or primitive discharge. It takes place most copiously from the edges of strips of metal, or from the surfaces of ragged or asperous wires; and to a greater extent in rare than in dense air. Hence, sharp-edged strips of metal, carrying a heavy flash of lightning, would necessarily discharge a great quantity of fluid from both edges to the neighbouring objects, or into the air.

24. The *third* kind of lateral force occasions a displacement of the electric fluid natural to those bodies, which are vicinal to a continuous conductor, carrying the primitive discharge.

The "balls of fire" seen "running about the deck" of the *Rodney*, with the flashes and sparks (12-13), were more likely to be occasioned by lateral forces of this kind, or by *waves* from the first kind (22), amongst the articles below, than being any part of the lightning which struck the ship. Electrical waves produced by a discharge through a conductor, situated close to the powder magazine, would produce intense sparks amongst the powder barrels, whose metallic linings and hoops would reciprocally exchange them.

* The *principal* discharge obviously passed through and over the surface of Hollingsworth's body; whilst Wilson was probably hurt, and thrown in the opposite direction, by the lateral explosion.

A COMPARISON OF THE OBSERVED EFFECTS OF LIGHTNING, AND THE PROBABLE EFFECTS WHICH LIGHTNING WOULD PRODUCE BY THE APPLICATION OF MR. HARRIS'S SYSTEM OF CONDUCTORS TO SHIPPING.

25. Were there no other data than those afforded by the *fusion* of the metallic sheave belonging to the *Rodney* (12-13), and the fusion of the chain conductor belonging to the brig *Jane* (20 note), we should have ample demonstration of the super-eminent calorific powers which lightning exercises on metallic bodies of considerable dimensions ; and of the probable effects producible on masses of still greater magnitude, by the same agency.

These specimens of electric action, when exhibited on the grand scale of nature, and which mock every human effort at competition, ought to be regarded as monitors of inestimable value, prominently and opportunely placed before us, as if commissioned by an all-merciful Providence to warn us of the imminent danger, or certain destruction, to which thousands of mortals may be exposed by the misplacing of lightning conductors in ships containing combustible materials. Let us, then, avail ourselves of these well-authenticated and most important facts, and endeavour to profit by the inestimable examples which they afford for our instruction and guidance. The impressions which these facts convey to the mind are too forcible and too definite to be easily misunderstood : they clearly imply that either of the discharges of lightning which struck the *Rodney* or the *Jane* would have been powerful enough to have rendered even the thickest part of Mr. Harris's conductors *sufficiently hot to ignite gunpowder ;* and that a similar discharge of lightning, striking the mizen-mast conductor, should it pass through the powder magazine, would probably be the means of the ship being blown to atoms. Hence it becomes obvious that the calorific effects alone, by a *direct* discharge of lightning, would be sufficient to put in the most imminent jeopardy every vessel which is furnished with conductors that pass through or near to the powder magazine.

26. It appears from Mr. Harris's list of cases (16) that the lower-masts are more frequently injured than the topmasts, and the topmasts more frequently than the topgallant-masts : hence, although the *Rodney* and some other ships have been struck *above* the topmast, it is obvious that lightning more frequently strikes the rigging below the topmast-head than above it ; and by taking into account the damage done by the mere falling of the topgallant-mast, as a consequence of the masts below it being struck and injured, it is highly probable that the cases in which lightning strikes the spindle at or above the topgallant mast-head, bear a very small proportion to the cases which lightning strikes the sides of the masts and yard-arms. From this

inference, it will be obvious that an *oblique* flash of lightning, striking the body of the rigging, could not arrive at a conductor let into the wood of the *after-side* of a mast without damaging the mast itself, unless it came from a cloud astern the vessel. Were the lightning to strike any of the yard-arms, in order to arrive at a conductor, that yard-arm would receive as much damage as if no conductor had been there; and it is even possible that the conductor would be the means of increasing the damage, by causing the lightning to run along the whole length of the yard-arm to the mast;* and the mast itself might then be traversed by the lightning, and shattered between the yard and the conductor. The sails, ropes, spars, and every article which the lightning traversed on its way to a mast, would receive precisely the same extent of damage as if no conductor were attached to it; and men placed in or near the track of the lightning would be as sure to receive a death blow as under any other circumstance in which lightning entered the rigging. Moreover, as these *central* conductors would offer increased facilities for lightning to strike the masts, all the evils usually attending oblique discharges, through the rigging to them, would necessarily be increased in like proportion.

27. To whatever species of force we may be disposed to allude the bursting of the massy iron hoops on the mainmast of the *Rodney* (12), we are compelled to infer that a similar force, operating on a mast with its conductor, would be productive of similar effects, to a certain extent. If we are to suppose that the hoops were destroyed by the force of a lateral explosion of the first kind (22), then a similar lateral explosion between the mast and its conductor would shatter both of them about the place where the lightning struck the mast; and it is not overstepping the boundaries of electrical evidence to infer that the *closeness* of the conductor to the mast would be one circumstance, at least, conducive to the destruction of the latter. The iron hoops on the mainmast of the *Rodney* were close jambed to the wood, and situated under the best circumstances to be split open from a sudden expansive force within. Upon the same principle, a conducting strip of copper, close jambed to the wood, within a groove in the mast, might probably not only be burst asunder, but peeled from the wood for many feet upwards and downwards, from the point where the lightning struck the mast.† No circumstance could better substantiate the veracity of this inference than the following, which is so clearly described in the accounts from the *Rodney*. "The copper binder round the truck was burst asunder, a small piece

* In May, 1847, H. M. S. *Dido*, 18 guns, fitted with conductors in the masts, received a flash of lightning upon the "extreme point of the main royal-yard-arm, and, in its course to the conductor on the mast, demolished the yard and tore into small pieces, or scorched up, the greater part of the sail."—*Harris on Remarkable Examples of the operation of Capacious Metallic Conductors.*

† On the 26th September, 1846, H. M. S. *Fisgard*, 44 guns, was struck by lightning, whilst moored off the Nisqually River, in the Oregon territory. "The next morning, on examining the conductor along the mast, the vane-spindle was discovered to be fused at the point, and blackened one-third of the way down. * * The conductor on the mainmast, low down,

broken out of the truck, and one of the metal sheaves for signal halyards slightly fused." In this case, the fused sheave was *within* the boundaries of the "copper binder;" and as it is highly probable that the lightning was divided between the two masses of metal in this part of its course, the force of the lateral explosion occasioned by that portion only which passed through the sheave was sufficient to burst asunder the exterior copper binder. If it should be discovered that the copper binder of the truck was sufficiently heated to render it brittle, such circumstance would certainly facilitate the rupture of the metal; but by no means invalidate the inference I have drawn respecting the effects of the lateral force; whilst, at the same time, it would be an additional proof of the astonishing calorific effects which lightning is capable of producing on the best metallic conductors. The manner in which the fragments of the iron hoops were magnetized, if we consider the lightning to have *descended*, would prove that it traversed the mast *within* the hoops, and its lateral force the cause of their being torn asunder (12-13).

28. The electro-magnetic effects occasioned by a flash of lightning, traversing conductors which pass through the hull of the vessel, would be exceedingly injurious to the chronometers on board, by magnetizing every piece of ferruginous matter which enters into their construction, and thus deranging their performance to an irremediable extent whilst those pieces remained in the instruments: not only would the *principal* conductor in the mast be productive of these effects, but every strap, knee, and other metallic appendage to that conductor would communicate *permanent* Magnetism to all those morsels of steel which form so essential a part to those valuable Horological instruments. The chronometers on board the *Beagle*, surveying ship, to whose safety Mr. Harris ascribes the instrumentality of his conductors,* are, by the circumstance of their not being injured, almost certain evidence that the ship was *not* struck by lightning at those times when the "hissing sound was heard on the masts" (14). Had a discharge of lightning traversed the mainmast conductor, every

was found to have been *started* (peeled) *from the mast in three places.* * * The plates of copper forming the conductor *were seperated* at the lowest point, and *thurst*, as it were, *asunder*, &c.—*Captain Duntze's Report.*

August 6th, 1843, H. M. S. *Sylla* was struck by lightning in the West Indies. "Some of the buts of the copper-plates in the topgallant-mast were *started* by it, and in one place buckled up at the edges; some of the fixings also were shook and loosened.—*From the Ship's Log.*

All these cases occurred subsequently to the first publishing of the Memoir, and are in perfect accordance with the predictions.

* "The report from the *Beagle* shows that the conductors had performed their office and defended the ship: this, it must be allowed, was of the utmost, perhaps vital, importance to the survey on which she was employed; since, if the vessel *had been struck* by lightning, it is more than probable that the many valuable chronometers, compasses, and philosophical instruments of various kinds, necessary to the survey, would have been seriously damaged—the cabin set apart for the charts not being far from the mainmast."—*Harris's State of the Question*, &c. Section E, page 7.

The above paragraph would imply that the *Beagle* was *not* struck by lightning; but as Mr. Harris has elsewhere given a different opinion (15), it is possible that this passage was intended to state that those instruments would have been damaged "if the vessel had been struck by lightning," *and not furnished with his conductors* at the time.

chronometer in the cabin would have been suffered from its electro-magnetic influence; and no plan or invention whatever, excepting such as would distribute the electric influence on every side alike, could possibly prevent chronometers from being subject to these magnetic effects.

29. Mr. Harris is of opinion that the light which appeared to run down the masts of the *Dryad* (14-15) was a decisive evidence of the ship being " struck by lightning in the common way, but without the ordinary ill consequences ;" and says, " the electrical agency seems to have fallen so powerfully on the masts that it produced about them a luminous atmosphere—a phenomenon not uncommon when large quantities of Electricity traverse conducting bodies, in a heated or rare state of the air, such as that usually found on the coast of Africa."*

30. It is true that rarified air, whether hot or cold, will facilitate the production of a luminous atmosphere around a conductor carrying an electric discharge ; but this is not the case with *unattenuated* air, though of a high temperature: on the contrary, heat offers a decided resistance to the display of the phenomenon, and confines it within narrower limits than it would otherwise expand to and occupy. Unless, therefore, it can be shown that the barometric column was exceedingly low indeed at the time of the occurrence, Mr. Harris's explanation is by no means applicable to the case. Neither would a luminous atmosphere, such as Mr. Harris calculates on, have any resemblance to the phenomenon described by Captain Turner ; for, instead of its appearing like lightning down the mast, the phenomenon would have been a momentary glowing column of electric light. We cannot produce anything like a *running light* when the conductors are sufficiently good and capacious to conceal the motion of the fluid, though such a phenomenon may easily be produced by the employment of inferior conductors.

31. I have seen a globe of electrical light traverse the surface of a wet silken cord upwards of three feet long. One end of this cord was tied to the lower end of a kite-string, in which a strand of copper wire was laid, and the other end was tied to a young tree. Some non-commissioned officers of the Royal Artillery, who were looking on, thought that the electrical globe was about the size of a musket ball. The lightning was very heavy at the time ; but neither the kite, which was only about fifty yards high, nor the string were ever struck by it. The hissing noise in the string could be heard at the distance of a hundred yards.

32. On one occasion, at Maidstone, a continuous stream of dense sparks traversed the same silken cord, when quite dry, for several successive minutes, whilst a cloud passed over the kite. The string was cut, or burnt, at about three hundred yards from the ground, and the kite lost; but no lightning was seen, nor thunder heard, at the time. More than fifty persons witnessed this fact. We heard thunder at a distance

* " State of the Question relating to the protection of the British Navy with Lightning Conductors."

about some ten minutes after the cloud had passed over the field where we were experimenting.

33. At Kirby Lonsdale, in the spring of 1834, a man, named William Croft, was severely struck by a discharge from my kite-string, when no lightning was present. The electric fluid discharged itself over a stick four foot long, which Croft held in his hand, pointing the farthest end to the string: a hail shower was falling at the time. Several hail showers fell the same day from well-defined insulated clouds, leaving the sky quite clear after the transit of each. Long, dense sparks could always be had at the kite-string on the approach of a cloud, and during its transit over the kite; but only very feeble electrical indication could be obtained when the sky was clear for some distance about the kite.

34. Under similar circumstances, at Addiscombe, I have had rapid, *spontaneous* discharges from a two-gallon jar, whose inside was connected with the kite-string.

35. I once received a severe blow from a kite-string when no visible cloud was within a mile of the kite, although the string, containing a wire its whole length, was uninsulated (tied to a tree) at the time, and at the distance of three feet from the place where I stood. The discharge took place over a dry silken ribbon, with which I was attempting to lower the kite. I had received several shocks before this heavy blow, which was so severe on my chest, thigh, and shin-bones, that I was deterred from taking down the kite until a cloud, which I had observed to windward, had passed over to a great distance on the leeward side. It was a thin ragged cloud, and did not discharge any lightning. This fact occurred in the Royal Artillery Barrack field, Woolwich.

36. I have been particular in describing these facts, because they show that those phenomena which Mr. Harris considers as sure indications of his conductors being struck by lightning on board the *Beagle* and *Dryad*, are more likely to have been the mere consequence of *electrical waves* produced by clouds or neighbouring lightning. Lightning invariably causes electrical waves, but it is not absolutely necessary to their production, nor to the production of the phenomena in question. And as Mr. Harris has not taken any such facts into consideration, they will appear more important in this discussion; because, if he had not been aware of them before, they may now possibly have a tendency to give him very different views to those which he has hitherto taken respecting the *cause* of the phenomena which were observed on board the above-named two vessels.

37. It is highly honourable to Mr. Harris, however, that he has, in the most liberal and open manner, given a fair and ample invitation to investigate "the state of the question relating to the protection of the British Navy from lightning"* by that system of con-

* "It is, therefore, further submitted, that Mr. Harris may justly claim to have these circumstances fairly considered. In seeking for an inquiry, he aims at nothing which may not come openly and fairly before the country, without any kind of reservation whatever."—*State of the Question, &c.*

ductors which he has proposed. At the present crisis, a more important scientific topic could not well be imagined, nor can there be one which has a greater claim upon the serious consideration of every experienced Electrician. I have been induced to respond to Mr. Harris's solicitation in the hope that, from my humble example, others more competent than myself, and with more ample means at their disposal, will be induced to take up the subject. I hope, however, that I have succeeded in pointing out some important truths, which previously were either not known or unaccountably neglected; and that I have been enabled to place the true character of Mr. Harris's plan of conductors, and the experiments intended for the illustration of their superiority over others, in a much clearer point of view than any in which they have hitherto appeared.

38. Having no motive, beyond that of the public good, for introducing his conductors to the Navy in preference to other systems, Mr. Harris will feel a gratification at his solicitations to inquiry being thus promptly and diligently attended to; and that this great question has been fairly and impartially discussed—that mere speculation has been studiously avoided—and that no inferences have been drawn but such as either rest on incontrovertable data, or have ample probability in their favour.

DESCRIPTION OF A NEW SYSTEM OF LIGHTNING CONDUCTORS FOR SHIPPING.

39. By the preceding investigation we are enabled to understand that narrow strips of copper, let into the masts, afford but a very partial protection to a great part of the rigging, and are liable to very serious injury from oblique flashes of lightning striking the masts (26-27); and that, in order to prevent all hazard of leading destruction into the very vitals of the ship, it is more prudent to conduct the lightning entirely away from the interior of the hull than directly through it, and as clear from the plane of the masts, and as exterior to the principal parts of the rigging, as circumstances will allow. Hence, therefore, had the frequent hauling up of chain or other flexible conductors to the masts' heads not been a serious inconvenience, those metallic chains, or ropes, if sufficiently numerous, would probably have been as secure a protection to vessels as any that could easily be devised; for instead of permitting an oblique discharge of lightning to traverse a portion of the rigging, with the probability of splintering a yard-arm, or a mast before it arrived at them, they would, by their *exterior* position, envelop within their boundaries not only the masts but the principal rigging of the vessel, and shroud them from the effects of the discharge: and no plan of protection to the masts, from a *perpendicular* discharge of lightning, could be more perfect than a system of continuous conductors hanging from the masts' heads, and

passing over the sides of the vessel into the sea. As, however, the hauling up of such systems of conductors, and the liability of their being an obstruction to the working of the sails, &c., are strongly urged as objections to their general use; and that *fixed* conductors are absolutely desirable, not only to prevent occupying the time of the men when wanted for other duties of the ship, but also to be in constant readiness to parry the effects of sudden or unexpected electrical storms, we should endeavour to employ all such means as are available to accomplish this great object; and as simplicity and security ought to be the leading features of every plan, and economy always kept in view, I am not aware of any plan so likely to meet the demands of the circumstances to so great an extent as the simple system of conductors which I now propose to describe.

40. Nothing can appear more simple than to attach fixed conductors to the lower rigging, because the lower-masts are always standing under every circumstance of weather. I therefore propose to protect the lower masts and rigging by cylindrical rods of copper, four to each mast, and situated exterior to the shrouds, having one *before* each fore-shroud and one *aft* each after-shroud—the upper extremities of these conductors to be attached to the fore, main, and mizen-tops, as distant from the masts as circumstances will allow, and in any manner most secure and convenient. The lower ends of these copper rods are to be fixed to the chains on the outside of the fore and aft shroud of each mast, and continued, by broad and stout straps of copper, to the copper sheathing of the vessel. By these means, both the *starboard* and *larboard* shrouds of every mast would each be flanked by two conductors, which would be always at their posts, ready to receive any flash of lightning tending towards the masts, from whichever side of the ship it might approach. As these conductors might be made to extend to some distance *fore and aft* of their respective shrouds, without obstructing the working of the lower sails, they would form an alinement on both sides of the vessel, so as to protect each other's masts as well as their own. To prevent lightning from entering the lower rigging from ahead, I propose a conductor on each side of the forestay, their upper ends to be united with the conductors of the foremast, and their lower ends with the sea, in the most convenient way. The whole of these conductors would be as permanent and as unobstructive to the working of the vessel as any part of the standing rigging; and, with the exception of the afterpart, would so completely enshield the lower rigging as to parry any flash of lightning tending towards it. The lower yard-arms would, however, still be unprotected, though not so liable to receive injury from without as by Mr. Harris's system; because, in every position, they would be near to some of the conductors, which would relieve them of any discharge they might receive before it arrived at the mast. Yard-arms would be very easily protected by flexible conductors, uniting them with those which are permanently fixed. By such a simple system of conductors, the whole of the

lower-rigging would be as completely protected from lightning as any system of *fixed* conductors are ever likely to protect it; and certainly without the slightest chance of injury to the lower-masts or to the hull of the vessel—for though there were no conductors in the upper rigging, and that lightning were to strike an upper mast, it would do no injury below the lower mast-head, as it would there enter the system of conductors, which would convey it over the sides of the vessel to the sea.

41. The topmasts and rigging are easily protected by a system of conductors similar to those already described for the lower-rigging, and may remain permanently fixed as long as the topmasts are standing. They may consist either of inflexible rods or of flexible metallic ropes, of sufficient dimensions, four to each topmast. The lower extremities of these conductors are to be united with the upper extremities of the lower ones, and their upper extremities to the cross-trees. The upper and lower systems of conductors may be so continuously united, by the former sliding on the latter, as to present no inconvenience whatever, even under circumstances necessary to strike or to remove the topmast. By these systems of conductors, every part of the ship below the topmasts'-heads would be well guarded, and without the slightest risk of danger from the presence of the conductor; and, as lightning but seldom strikes the rigging higher than the cross-trees, it would not be of much consequence to carry the conductors higher up. Oblique flashes of lightning, which might otherwise strike the topgallant-mast, would be directed to the topmast-head, when furnished with these conductors.

42. As, however, every chance of danger to the men, and every species of damage to the vessel, ought strictly to be avoided as far as practicability will admit, it still appears desirable to furnish the topgallant-rigging with conductors; and perhaps those which would give the least trouble to the men would be strips of copper let into the grooves in the masts, according to the plan proposed by Mr. Henley; but, instead of *one* strip only to each mast, I should propose *three* in each, at equal distances from each other—which, by having an exposure of metal on every side, would be a greater security to the mast than by its having one strip only. These strips of copper need only be narrow, and of moderate thickness, as they would be made to act in concert by uniting them at, or near to, the truck with a copper band: a similar band would unite them at the lower end of the mast. Precautions for maintaining metallic contact between the topgallant conductors and the system next below, must necessarily be attended to. The strips of copper should be continued to the vane spindle, which should be surmounted by a large ball, and not by a point as hitherto.

43. Four cylindrical copper rods, or four flexible metallic ropes, stretched from the cross-trees to the truck, parallel to the topgallant-shrouds, would afford a much better protection to the topgallant-rigging than any conductors let into the mast. Flexible conductors would be as easily set up, or taken down, as the shrouds themselves.

44. The bowsprit would be very well protected by the conductors accompanying the forestay (40), they being continued on each side, near its lower surface, to the sheathing at the bows; and the jib-boom would be protected by three copper ropes or chains, reaching from its extremity to the conductors of the bowsprit. The jib-boom, however, being so little liable to be struck by lightning, presents no very strong claims to attention.

45. Let Fig. 3, Plate XIV. represent a transverse section of a vessel through the plane of a mast. Then the lines, *c c*, *c c*, will represent the conductors on the *after-side* of the lower shrouds; and the inner lines, *c' c'* and *c' c'*, the conductors on the *fore-side* of the lower shrouds. The lines, *t t* and *t t*, will represent the conductors on the *after-side* of the topmast shrouds; and the lines, *t' t'* and *t' t'* will represent the conductors on the *fore-side* of the topmast-shrouds. The conductors of the topgallant-masts are too easily understood to need further illustration.

46. Having thus described a system of marine conductors which, to me at least, appears more efficient and less objectionable than any other hitherto offered to public notice, the next consideration would be the expense of equipment, which I will compare with the expense of equipment by Mr. Harris's plan. The official document,*

* Documents Relating to Expense.—The following are extracts from a report by a gentleman who was sent from His Majesty's Dock-yard, at Chatham, by order of the Commissioners of the Navy, to estimate and inspect the work; who, being a naval engineer, and otherwise a person of great intelligence, was considered by them equal to the task :—

"His Majesty's Dock-yard, Chatham, July 9, 1834.

"Sir,—In obedience to instructions received from the principal officers of this yard, in conformity with your official communication of the 1st instant, I herewith enclose a statement (in a condensed form) of what would be the expense, in labour and materials, of applying Mr. Harris's lightning conductors to each class of ships, supposing them fitted at the most eligible time. I have likewise ventured to append a few cursory remarks, which seemed necessary, in order to afford a criterion of what may probably be the *ultimate expense* to the public, in the event of the plan being generally adopted in the service. With this view, I beg further to transmit schemes of prices for labour, for different artificers employed on the work, upon which it should be added the accompanying estimates has been made.—I have the honour to be, &c. &c.

"To G. J. Smith, Esq. Secretary to H. M. Navy, &c." "W. M. Rice.

Mr. Rice then proceeds to give a complete table of particulars in fitting each part of a ship, according to the plan above mentioned, the whole of which it is not necessary to detail here. It will be sufficient to state the general results, as they appear in the last columns of his table :—

TABLE 5.

Class of Ships	Total of Masts and Hull.					
	Labour, &c.			Reconvertible Copper Materials.		
Guns.	£	s.	d.	£	s.	d.
120	60	18	0	305	12	2
84	56	18	0	292	19	11
74	54	17	0	263	2	3
50	50	16	0	235	17	3
46	45	15	0	190	16	4
28	38	14	0	123	13	3
18	29	12	6	89	14	8
10	24	10	0	77	16	9

shown in the note, which is copied from Mr. Harris's pamphlet, is highly valuable on this point; coming from the best authority, it furnishes unexceptionable data.

47. Mr. Harris has given the following dimensions of his conductors for "one mast of a frigate of fifty guns:"*—

	Length. Feet.	Length. Inches.	Mean Width. Inches.	Cub. In.
On the royal pole	18	3	2	82
On the topgallant-mast	17	0	2·5	95
On the topmast	50	0	4	450
On the lower-mast	93	0	6	1255
Total copper				1882

48. By taking the specific gravity of copper at 9,000, the weight of 1882 cubic inches will be 613 lbs. nearly—which, at one shilling per pound (and it may be had at a lower price), would cost £30 13s. This average for each mast would give £92 nearly, the cost for copper on the three masts of a fifty-gun frigate.

By referring to Mr. Rice's estimate for a similar fifty-gun ship, we find that the copper alone costs nearly £236. Then, 236—92 = £144 is the cost of the appendages, mostly below deck, to Mr. Harris's conductors.

There is still, however, another consideration in these estimates. Mr. Harris's conductors proceed to the bottoms of the masts, and are the broadest below deck; therefore the quantity of copper *above deck*, in each mast, is considerably less than 1882 cubic inches, and its cost proportionably less than £30 13s.

49. Now the quantity of copper on the lower mast is about two thirds of the whole (47); and, by allowing about one-third of that on the lower-masts to be below deck, we have $(2 \times 92) \div 9 =$ £20 9s. for the cost of the copper in the masts below deck. Rejecting the nine shillings, we have 92—20 = £72 for the cost of the copper in that portion of the copper conductors which are above deck.

Now, this seventy-two pounds-worth of copper (49) is, of course, supposed by Mr. Harris to conduct lightning safely to the deck; then, as my system of conductors on each mast would act in concert as decidedly as if in one piece of metal, they, with the *same quantity* of copper, would stand the same chance of being efficient to the *chains* as Mr. Harris's to the deck; and consequently the cost of the metal for my conductors would be £72, with the additional cost of about £15 for the forestay conductors, and fastenings to the whole.

Upon this report Mr. Rice offers the following remarks :—

"This estimate is grounded on the supposition that Mr. Harris's plan be applied to ships at the most eligible time—viz. during the progress of building or repairing; when the essential or original fastenings of the hull may be made to subserve as conductors. Much delay will be thus prevented, and many contingent expenses saved."

* Nautical Magazine, for November, 1837.

51. The quantity of copper above deck in Mr. Harris's system does not appear to me to be sufficient, requiring at least one-half more. Hence the cost of the copper for the equipment of a fifty-gun frigate, with my system of conductors, would be £130, calculating it at the rate of one shilling per pound, which is perhaps about one-sixth too much; and consequently the material for Mr. Harris's system would cost more than double the sum required for mine.

52. The next thing to be considered is the expense of workmanship, which, for my system, could not possibly exceed £20—which, being deducted from Mr. Rice's estimate of £50 16s. would leave a saving of £30 16s. But Mr. Rice's estimate is "grounded on the supposition that Mr. Harris's plan be applied to ships at the most eligible time—viz. during the progress of building or repairing, &c." (46 note); and consequently applicable under those circumstances only. Under all other circumstances the expense would be enormous, the ship having to be completely dismantled above deck, and the lower masts taken out and removed from her. It appears, therefore, from the whole, that under the best circumstances in which Mr. Harris's conductors could be applied to a fifty-gun frigate, the expense would be £286 13s. To equip a similar vessel with the system which I have proposed would never exceed £150, reckoning the copper at one shilling per pound. At tenpence per pound, the expense would be £128. Taking the mean of these prices, the expense of the two plans would stand as below:—

Harris's system	£286
Sturgeon's system	139
Difference	£147
For a 120 gun ship the difference would be nearly	£200

53. The *time* necessary to equip any ship with Mr. Harris's system of conductors would be ten times that necessary to equip a similar ship with the conductors which I have proposed. Moreover, the latter system could be fitted to any ship, on any station or on any service, either at sea or in harbour. I do not, however, advocate the cause of economy as a primary object; as the only gratification which I can experience must emanate from a consciousness of having made a proper application of the principles of science to the protection and welfare of my fellow men.

W. S.

Westmoreland Cottage, Old Kent Road,
London, Sept. 8th, 1839.

P.S.—Since my fourth Memoir was first published, I have been favoured with the opinions of several naval officers respecting my plan of protecting shipping from lightning; and having availed myself of some valuable suggestions as to the probability of *those branch conductors*, which I had proposed to be placed *before* the shrouds of each mast, being liable to injury by pressuse from the yards when the ship is close hauled, I have dispensed with them altogether. The system will thus be much simplified, and give an opportunity of placing the whole of the metal in those conductors which are *aft* the shrouds, and entirely away from the masts. This plan will also remove every apprehension which might have been entertained of the shrouds being ignited in consequence of lightning heating those conductors which were in partial contact with them.

Fig. 3, Plate XIV. represents a vertical section of the *starboard* and *larboard* conductors of each mast, from the topgallant-mast-head to the copper sheathing of the vessel; and the darts show the manner in which a flash of lightning would be conducted down both sides of the rigging to the sea. Should a flash of lightning happen to strike either of the topgallant-mast branches, then, because of their metallic union above, at the mast-head, the lightning would be distributed through both branches as decidedly as if it had struck the spindle above the truck. The topmast conductors, being metallically connected at the cross-trees, would each conduct its own share of any flash of lightning which should happen to strike on either the starboard or the larboard branch. The lower-mast conductors would also reciprocally and mutually assist in conducting lightning which should strike either of them. Hence, therefore, on whatever part of a conductor lightning should strike, it would invariably find two conducting channels to the sea, one on each side of the ship.

Fig. 4 gives a side view of the starboard and forestay conductors of a three-masted vessel; also a view of the main topmast and mizen topmast-stay conductors, which might be applied if thought necessary. These latter would be the means of uniting all the conductors into one system, from the topmasts'-heads downwards to the sheathing. By this means, every flash of lightning striking any branch conductor of the system would find its way to the sea through seven conducting channels, three on each side of the ship, and one from the forestay conductor, which terminates under the bows. The small darts in Fig. 4 show the distribution of the electric fluid from a flash of lightning, supposed to strike the main-topgallant mast-head, or the vane spindle above it. Lightning thus extensively distributed amongst distant branches of the conducting system would be rendered perfectly harmless.

The fastenings once completed, the conductors could at any time be put up in a few hours, and remain stationary during a cruise or a voyage as decidedly as the standing rigging.

AN EXAMINATION OF VARIOUS THEORIES ON ELECTRICITY.*

TWENTY-SECOND MEMOIR.

PART I.

(Read December 5th, 1837.)

Different opinions of Philosophers respecting the Nature of Electric Action—The Vibratory Hypothesis examined—Its Principles not Analagous to those of the Hypotheses of Sound and Light—Evidences of the existence of an Electric Matter.

1. The Memoir which I am now about to offer to the notice of the Electrical Society may be considered as the first of a series which it is my intention to bring forward as speedily as circumstances will permit. These Memoirs will exhibit a selection and arrangement of facts, which, if I have not deceived myself, can hardly fail to have some weight in the reasonings of those philosophers whose opinions are not yet reconciled to each other respecting the *modus operandi* in the production of certain electrical phenomena.

2. In an enquiry of this kind, it often happens that, notwithstanding the apparently trivial circumstance to which it is mainly directed, it becomes essentially necessary not only to notice but to investigate certain other points with which it is obviously connected, in order to satisfy the mind respecting the bearing and influence which those points have upon each other; as also on that which is the principal object of pursuit.

3. Philosophers of the present day have taken such extremely dissimilar views of the nature of electric action, that they have imposed upon us the indispensableness of a minute retrospection of almost all the variety of phenomena that have hitherto been developed, the laws of whose exhibition necessarily contribute to the establishment of those impenetrable and unalterable principles upon which the science must ultimately rest; and have demanded a re-investigation of even the very arcanum of all electric action.

4. A certain class of these philosophers who have undertaken the explanation of electric phenomena, consider it requsite to combine the operations of *two* distinct kinds of electric matter, which they have called the *vitreous* and the *resinous*, inde-

* From the Transactions of the London Electrical Society.

pendently of which, they imagine no electric phenomena can possibly exist; a second class have contented themselves with the management of *one* fluid only; whilst a third class, still more economical than the preceding, have undertaken the solution of every electric problem hitherto discovered, independently of the operation of any electric matter whatever, by supposing that electric phenomena are the effects of certain rotatory or vibratory motions of the particles of the common matter composing those bodies on which they are displayed. Each of these hypotheses is supported by men of the highest respectability, and of acknowledged talent. They are become subjects of much important discussion amongst Electricians of every country, and therefore a rigid and impartial investigation of the principles on which they are founded is the only mode of proceeding which can satisfy the mind as to the intrinsic value of their respective peculiarities, and to form a proper estimate of their individual claims to attention. The results of such an inquiry, if properly conducted, can hardly fail to be interesting to the Electrical Society, whilst in the present instance, it may be regarded as an important preliminary step unavoidably touched upon in the path of research I have ventured to pursue.

5. In an undertaking of this magnitude and importance, under circumstances embracing a balance of authority amongst those who have taken these very different views of electric action, much caution and rigorous circumspection ought necessarily to be observed in every stage of the inquiry. Moreover the present infantile state of this Society imperatively demands that a copious selection of obvious and unequivocal experimental data be advanced, and that all reasoning therefrom be plain, lucid, and familiar.

6. Respecting the order in which these hypotheses come under consideration, the precedency would have been but of very little consequence had the probability of truth appeared equally favourable amongst them, or that they had differed from each other in some trifling peculiarity of detail only; but as there is such a wide difference in the very bases of these doctrines, and especially between those which admit of the existence of an electric matter, and that which precludes it altogether, it appears essential that the mind becomes perfectly satisfied, as early as possible, respecting the nature of the evidence on which these opposite theoretical views have been founded, and by which they are the most likely to find support, in order that the investigation may be facilitated by disposing of those first which appear to have the least probability in their favour. Moreover, as the existence of an electric matter has, for a long series of years, been acknowledged by almost every philosopher who has paid a sufficient degree of attention to the subject to enable him to form an unbiassed opinion, resting on experience alone; and that the vibratory hypothesis appears more like a novel creation of the imagination than as a doctrine founded on observation and fact, and still remains little more than a confusion of discordant surmises, without

even the least pretension to systematic organization, or the shadow of either law or rule for the guidance of the Electrician; there can appear no impropriety in commencing with a brief inquiry into the extent of interpretation of electrical phenomena which the latter hypothesis is capable of affording, and afterwards examining that which has so long rested on the supposition of the individuity of an electric matter, and which has obtained a code of laws, supposed to be sufficiently explanatory, as far as they proceed, of every fact hitherto developed in certain branches in Electricity.

7. If the hypothesis, first for consideration, supposes that electric action depends upon, and emanates from, vibratory movements of the particles of those kinds of matter of which the apparatus employed are usually constructed, such as metal, wood, glass, &c., independently of any other agency, there could be no difficulty in showing its entire fallacy; because it is a fact easily ascertained, that no very energetic electric action, if any at all, can possibly be exhibited by any vibratory movements which those bodies are susceptible of receiving by mechanical means, although those vibrations might be infinitely greater than could possibly be produced by any other mode of procedure. Nor can I think it possible that any Electrician of the present day, whatever may be his theoretical views, or however fond he may appear to be of novel modes of explanation, would be found so far deficient in electrical knowledge as to hazard his fame by an attempt to charge a Leyden jar by the mere vibration of a mass of copper, glass, wood, or any other solid body whatever.

8. If again it can be supposed that the charge of a Leyden jar, or any other piece of glass, consists in certain tremulous motions of one or more of the materials of its structure, communicated from the prime conductor of a machine, is it possible to imagine that those motions would continue during the weeks, nay, even the months, that jars have been kept in an electrized condition after they had ceased to be in connection with any electric apparatus? There is no evidence of such continuous quiverings of the glass, nor is there a fact known to induce a belief that any tremblings exist in the instrument, even for a few moments after it has been taken from the machine; nor indeed any whatever, only such as are occasioned by the unavoidable shaking attending the working of the apparatus.

9. Can any one persuade himself that the electric action exhibited by two morsels of metal, by simple contact only, is occasioned by tremulous motions communicated to the metallic particles by the pieces just touching one another? Can he, moreover, stretch his imagination so far as to satisfy himself that the metals constituting a dry electric column will continue to quake for years after the instrument is first made; and that, notwithstanding all the care that is taken to prevent any motion amongst the materials of the pile, the electric phenomena exhibited by it are the mere effects of a tendency to motion which these materials naturally possess, and which keeps them trembling in spite of all the efforts of the workman, and contrary to the laws of inert matter?

10. I am well aware that the favourites of this hypothesis build much of their reasoning upon supposed analogies, particularly from the doctrines of sound and light, wherein it is considered that all the variety of phenomena constituting accoustics and optics is the effect of undulatory motions of matter, which in itself is neither sound nor light. But it must be borne in mind that sound is not produced without a first cause; the sonorous body must be agitated before it can be productive of sound, and the surrounding medium must be agitated to the ear in order to convey the proper impressions to the tympanum, otherwise the sensation of sound cannot exist.

11. Light also requires a first cause to shake the medium which is to be productive of it. Moreover, the medium itself is supposed to be *peculiar*, and alone appropriate to the exhibition of the phenomenon of light. Hence it is obvious, that unless the electro-undulatory theorists admit of the tremulous motions of some peculiar species of matter, they find but little support from analogy as far as regard sound and light. And an unequivocal concession of this point would dispossess the hypothesis of the most material peculiarity discoverable in its structure, by admitting the existence of an electric matter; and whatever appellation might be conferred on it, the very idea of its existence, and of the indispensableness of its presence in the production of phenomena, would be sufficient to assimilate this hypothesis with those it was intended to subvert; or if there could be any real difference, it would consist in the substitution of *vibrations* for *transmissions*.

12. As it is not my intention to enter into a historical account of the respective hypotheses which have been contrived for the explanation of electrical phenomena, it will be unnecessary to bring forward the names of individuals who have attempted to support that to which they have been most decidedly attached. Moreover, the hypotheses of Du Fay and Franklin are so well known to Electricians, that it would be quite unnecessary, at the present day, to go through a detail of the principles on which they are respectively founded; and, although we are very differently circumstanced with respect to the vibratory hypothesis, there being no work in which its principles are intelligibly developed, and consequently no source from which much useful information can be collected, it would be idle to discuss phantom theories which no one would claim; and which, in consequence of the vague indeterminate manner in which they have been ventured upon the credulity of philosophers, it would be difficult to trace to any origin by which they could not, without any refined sophistical dexterity, be very easily evaded. Indeed, I am not aware that any philosopher has attempted to lay down a *plan* for an electrical hypothesis in which a peculiar species of matter is entirely dispensed with; although there are some of them of considerable eminence who have ventured an opinion, that the existence of such matter is not essential to the production of electrical phenomena—assuming that

some *undefined* motion amongst the integrant particles of those bodies usually employed in the experiments are alone sufficient for the explanation. It would be exceedingly difficult, if not totally impossible, to form a rational idea of any mode by which such motions of the particles of solid matter could possibly exist. Such an idea appears insusceptible of demonstration, and bears not even the slightest token of probability. But admitting even the existence of these supposed motions, under certain circumstances, they would have to be regarded as *effects* rather than *causes*, which, like all other effects, would be referrible to some pre-existent force, however mysteriously that force might operate, or however its operations might be concealed from mental perception. The friction suffered by the revolving glass, or the stationary cushion of the machine, might possibly be construed into a cause sufficient to produce the supposed atomical motions; but its efficacy in this respect would require an elaborate strength of the imagination to continue it to electrized jars long detached from the prime conductor, and far removed from all other electric apparatus.

13. Any attempt to trace the polarity of the dry pile, or the electric condition of a still atmosphere, or any other electro-statical phenomena to the supposed restlessness of the particles of the metals, or of the air, would have to be ventured under the most unfavourable circumstances that could possibly have accompanied it—viz. in the absence of both fact and analogy. Every vertical column of a dry cloudless atmosphere, whatever may be its dimensions, is constantly electro-polar in one and the same direction, having its positive pole upwards.* But it would be difficult to reconcile the mind to a belief that this circumstance is solely owing to the intestine motions amongst the particles of the air, and independently of any other agent. Again the two sides of a single piece of thin metal, the one bright and the other dull,† or both bright but of different degrees of polish, are as decidedly electro-polar as the two surfaces of an electrized jar, or of the two extremities of the most extensive Voltaic arrangement; and it appears to be of little consequence how thin the metallic piece may be for this development of electro-polarity, but it would be ridiculous in the extreme to refer this circumstance to the imaginary quiverings, or rotations of the metallic particles constituting the piece. I have now in my possession some hundreds of pieces of thin zinc, each of which has had its two surfaces in opposite electrical conditions for more

* I have made more than five hundred experiments with kites for exploring the Electricity of the atmosphere; and in every case, when clouds do not interfere, I have found the upper strata positively electrical with reference to those which are below. I have had three kites, and sometimes five, at different altitudes at the same time, and have transmitted sparks from one to another from the top to the bottom of the series. In every case I found the uppermost of a pair to be the positively electrized stratum of the atmosphere; so that if the strata in which the kites were immersed were at altitudes corresponding to the series 1, 2, 3, 4, 5, their relative electric states would be very conveniently represented by those numbers. The experiments were made at different seasons of the year, and in every part of day and night. When clouds interfered, the distribution of Electricity natural to an unmolested atmosphere is often disturbed, and other phenomena occur which will be more particularly noticed in a future Memoir.

† See my Recent Experimental Researches in Electro-Magnetism and Galvanism, part 1, 1830.

than ten years; but I have never attributed their electro-polarity to any quiverings or other motions of the metallic particles, nor can I conceive that such an idea is possible to be formed either from any direct fact, or even from the most remote analogy. Light certainly pervades solid transparent bodies, but it is not considered essentially necessary that the solid particles of those bodies should become agitated for its transmission; or if such intestine motions of the transparent body were even required to accommodate the theory to the fact, still they would have to be acknowledged as effects and not causes; and even the waves of light themselves, which produce those effects, would have to be traced to a still more remote cause, which itself might be discovered to be an effect of the primitive disturbing force.

14. The ticking of a watch, or the scratching of a pin at the remote end of a long piece of timber, is more distinctly heard by an ear placed at the other end than if no such substance had intervened; but whether the explanation of this fact were to rest on the supposition of the impressions being communicated to the ear through the medium of the air in the capillaries of the wood, or on the incomprehensible vibrations of the solid mass, the immediate causes of those movements would still have to be referred to those of the watch or the pin, and those again to the respective forces which put these articles into motion. Hence in these cases, as in all others that I have had occasion to notice, the phenomena of light and sound are to be attributed to extrinsic causes, there being not the slightest evidence in favour of the supposition that either class of phenomena is a mere consequence of an innate corpuscular motion of those media which immediately transmit the appropriate impressions to the respective organs of sight and hearing; and consequently no analogies discoverable from which an idea could be formed of any innate atomic motions of the metal being the cause of electric-polarity in the pieces of zinc already mentioned (13). Having mentioned the motions of a watch as being referrible to another cause which is obviously traceable to the main-spring; and as this part of the machine appears to exert a force of its own accord, it may possibly be imagined that the hypothesis I have been discussing would find a favourable analogy in that circumstance. But it must be borne in mind, that unless the spring had been first moved by some other force, it would never have exerted any power over the other works of the watch; for whilst in its original form, and unmolested, it is perfectly inert. Hence the assumptive analogy again fails, and I believe no analogy is to be found within the precincts of physical science.

15. Whether electric phenomena be regarded as a mere variety of an extensive class, including those of light, heat, and Magnetism, or as a distinct kind traceable to an individual source, the probability of an electric agent would be very great, and much favoured by analogy; but there are other sources of information of much greater importance from which inferences may be drawn, and satisfactory conclusions formed,

on this part of the the electrical hypothesis. The indications of the existence of an electric matter are so various and extensive, that one would almost wonder how any idea to the contrary could possibly have been formed. The mechanical and calorific phenomena of Electricity are those which are most usually recognized as the productions of an electric agent, although I believe there are none hitherto developed that might not as easily be traced to the same cause. The displacement of granular substances, the perforation of compact bodies, and the fracture of those which are brittle (such as glass), and the violent blows given to animals by electric discharges, are all indicative of the action of some agent of considerable mechanical force. Moreover, these effects can be augmented almost to any extent, or they may be abated so as to be scarcely discernible, and this under circumstances wherein it would be said, in electrical language, that the same quantity of fluid were in motion in every case. If, for instance, the electric force excited by fifty turns of a machine were to be collected in a high state of intensity on the surface of a jar, and afterwards discharged in the usual manner through a metallic circuit, in an opening of which was placed a man, a pile of card paper, or a granulous substance, such as loose sand or gunpowder—a violent blow would be given to the man, the pile of paper would be perforated or even torn to pieces, or the gunpowder would be blown away from the spot without ignition; each fact indicating a mechanical force of considerable intensity, and even of the transmission or passage of some material agent. But if instead of the whole circuit (unoccupied by the man, paper, or gunpowder), consisting of metal, a portion of it, amounting to five or six inches, were of a thin strip of water, or a moistened thread of cotton, silk, or any such material, no such mechanical effects would be produced: the man would experience no shock, the paper would not be torn, nor would the gunpowder be scattered as before, but it would now be set on fire.

16. It would be exceedingly difficult to reconcile these phenomena to any self-vibratory motions which could be imagined to exist in the materials of the circuit, or to any motions of this kind pre-existing in the jar and transferred to them; but by admitting the existence of an active electrical agent, distinct from the solid and liquid parts of the apparatus, we are able to find an easy and natural solution to the problems which these varied phenomena present.

17. By assuming that the fifty turns of the machine forced a certain measure of the electric matter on to the surface of the glass, and that through the metallic circuit this matter was enabled to move with great celerity, we have then all the data necessary to satisfy the conditions of an electro-momentum of great energy, which is amply manifested by the effects it produces. But, on the other hand, when the same quantity of the electric matter is transmitted through the moistened thread, or any other inferior conductor capable of retarding its velocity, the momentum would obviously be lessened upon the strict principles of matter in motion; and the mechanical effects

upon bodies placed in the circuit would be proportionally abated, which is conformable to the results of the experiments.

18. If it can be imagined that with the metallic circuit the vibrations were more powerful than when a part of it consisted of inferior conductors, and that the mechanical action was increased accordingly, we should be under the necessity of allowing that the less degree of vibratory motion is essential to the ignition of the gunpowder. But how should we be able to reconcile this latter conclusion to other facts in which calorific effects of electric discharges are so eminently displayed? A piece of steel wire is ignited by an electric discharge under no other circumstances than when the entire circuit is metallic, or at least of very good conducting materials; and the more completely is the conducting powers of every other part of the circuit maintained, the more probable it is that the thin steel wire will be ignited. If the steel wire be short, it may be fused by a discharge from a moderately sized jar; but if it be long, a similar discharge would not make it visibly red-hot; and by having a moistened thread in the circuit, the thin steel wire would develop no conspicuous signs of even an elevation of temperature, although, as has already been shown, the latter conditions are those alone under which the gunpowder will ignite. From these facts we are led to understand that different inflammable substances require different modes of treatment to accomplish their ignition by electric agency; whilst, in a mechanical point of view, the character of the substances operated on requires no peculiarity of circuit for the production of similar effects—for invariably the mechanical action is greatest with the best conducting circuit, and abates gradually as the circuit becomes less and less perfect in its conducting character.

19. At the time I was making my experiments on the ignition of gunpowder by electric discharges, I was well aware of the necessity of varying them in every possible manner that I could think of; and the result of one of these experiments appears to have led to some doubt of the correctness of the theory which I then advanced to account for the cause of the action, the principal part of which may be embraced in a few words as follows:—When the discharge is made through good conductors—as copper wire, for instance—the electric matter passes through the gunpowder with so great a velocity that it has not *time* to ignite it; but when that matter is retarded in its progress, by having to traverse inferior conductors, the *time* occupied to pass through any transverse section of the circuit is sufficiently great to accomplish the ignition.* This explanation has been objected to, because it is a fact that in whatever part of the circuit the wet string formed a part of it, the gunpowder invariably ignited; and because the ignition was accomplished when the wet string was placed on what is usually called the *negative* side of the gunpowder, it has been thought that

* See my papers on the ignition of gunpowder by electric discharges. London Philosophical Magazine, vol. lxvii., and vol. i. of the United Series of the Philosophical Magazine and Annals of Philosophy. Also eighteenth and Nineteenth Memoirs.

the string could have no part in retarding the motion of the electric matter whilst traversing the gunpowder. This objection, however, may be very easily removed by assuming the electric matter as a highly elastic fluid, and contemplating the phenomenon in question upon the principles of elastic fluids generally. If, for instance, a a reservoir of condensed air were to be discharged through a tube sufficiently wide to offer little resistence to its motion, it would rush through the tube with considerable velocity, driving before it, of course, the air of the common density with which the tube was previously filled; or, in other words, the same quantity of air as that which was liberated from the reservoir would occupy but very little time in passing out at the farthest end of the tube. But if a similar reservoir of air were to be discharged through the same tube, now terminating with another of narrow bore, the time occupied for the escape of the air would be much greater than in the former instance; and precisely the same period of time would be occupied if the small tube were in any other part of the circuit, provided the whole of the air had to traverse it. If now this reasoning be transferred from the fluid air to the electric fluid, it will lead to similar conclusions, and show that if the velocity of the fluid be checked in any one part of the circuit it will also be cheked in every other part of it.

20. That the time occupied for a discharge through the two kinds of circuits is different, being much greater in one case than in the other, is so exceedingly obvious that no one acquainted with the experiments would attempt to deny the fact. Notwithstanding, however, it may possibly be necessary in this place to mention some of the appearances and effects by which it is most easily attested and understood. When the circuit is completely metallic, with the exception of a small opening in one part, for the purpose of examining the electric light, the jar is completely discharged by the shortest possible contact of the discharging rod; or, in other words, by a mere momentary closing of the remaining part of the circuit, and the electrical light at the opening is seen but for a moment, is exceedingly brilliant, and attended with considerable noise: but when the wet thread forms a part of the circuit, the jar is not so suddenly discharged, the light is seen for a considerable time at the opening, and its former brilliancy has entirely disappeared, being now reduced to a mere redness, and attended with scarcely any noise whatever. If, whilst the circuit was metallic, a finger were placed in the opening during the discharge, the finger would receive a severe blow, and for a moment be highly illuminated within; but by employing the aqueous circuit, no illumination would take place, neither would anything like a blow be experienced. The mechanical action of an electrical discharge is so far abated, by having a portion of the circuit of moistened thread, that the most delicate child might be placed in its way without experiencing the least inconvenience, indeed scarcely any sensation is discernible, yet in the very same circuit gunpowder might be ignited. I have frequently placed young persons in such a

circuit during the discharge from six square feet of coated surface on each side the glass, charged to a high intensity. These persons never experienced any disagreeable sensations from the electric action, although eight pieces of miniature ordnance, placed in other parts of the circuit, have been discharged by the same electric influence. But if the circuit were to be completely metallic, a similar electric discharge would produce such a violent blow that the stoutest man would not like to experience the sensation a second time.

21. In all these phenomena we have unequivocal signs of the existence of an electric matter, whose mechanical effects are as decidedly modified by varying its velocity, as are those of any other species of matter whatever, and whose light also is more and more intense as its density increases, but which becomes faint as it is attenuated, and viewed in a less compact body.

22. I am well aware that some philosophers are of opinion that the brilliant light which is developed by the electric spark, may possibly occur from a sudden displacement and subsequent collapsion of the atmospheric air, the caloric of which becoming sensible and luminous by compression; founding their reason principally on the fact, that the air, when discharged from an air gun, produces light. Now, admitting this to be the case, no one could suppose that these effects are produced independently of an adequate force; and a physical force invariably implies the existence of active matter. But we have no idea—certainly no proof—of a piece of glass or a piece of metal being thus active; hence we are constrained to admit of the existence of some other agent in the production of these phenomena. Moreover, it is well known that although the electric light is not so brilliant in attenuated air as in that which is more dense, its existence is still manifested in a very striking and beautiful manner. Our imitations of the Aurora Borealis are highly demonstrative of lumuinous electric matter. Besides, if there can be any discernible analogy in the light given by an air gun, and that of an electric spark, it must certainly be exceedingly remote, and the mind in which it is formed susceptible of more delicate impressions than that generally implanted in man.

23. The luminous phenomena calculated to attest the existence of an electric matter, are various and extensive. The pencil, the star, the cascade, the falling star, and the electric meteor, exhibited over the surface of moist conductors, are amongst those which appear insusceptible of explanation independently of an electric matter; and the splendid bow between charcoal points attached to the poles of a Voltaic battery, is untraceable to any other cause.* No one, at the present day, doubts the

* There is another beautiful phenomenon of electrical light which is not so familiar to many persons as those mentioned in the text. It is the *luminous electro-sphere* exhibited on a positively charged conductor. The experiment is made in the following manner:—Let Fig 1, Plate XV. represent a glass receiver, furnished with a collar of leathers, and a sliding rod passing through them. The rod has a brass ball at each extremity. This receiver is to be placed on the plate of an air pump,

identity of Electricity and lightning; and can there be a mind sufficiently impervious to external impressions, as to doubt of lightning consisting of, or emanating from, a peculiar and active agent? And what physical agents are there which admits not of materiality, either directly or indirectly? And what known agent but the purely electric is endowed with the activity and energies of lightning? Or capable of producing those tremendous effects universally acknowledged to be attributable to this power alone? I am well aware that some philosophers are more prone to *doubt* than admit any theoretical point which they themselves have had no share in establishing; and the philosophical reputation of some men rests principally on a steril system of *doubting*, which they gravely and inflexibly pursue. But even the most sceptic are sometimes led *indirectly* to an acknowledgment of theoretical explanations, which their proneness to doubting would not allow them directly to admit. Every philosopher who has contemplated the phenomenon, traces the *immediate* cause of thunder to a sudden collapsion of displaced air. The acknowledgment of this undisputed fact admits, without further evidence, of the existence of an electric matter which first displaced the atmospheric air; and however reluctantly we might be inclined to concede to the fact, the long line of space from which the sound originally proceeds constrains us to believe that the vacuum was not limited to a point, or to a small sphere of space, but that it was elongated and produced suddenly and without interruption, at very different distances from the observer; implying thereby that the matter which

from the centre of which rises a stout brass wire stem, surmounted with a ball. The ball, *b*, is to be adjusted at about five inches distant from the ball *c*, and the air in the receiver to be attenuated by the action of the pump. If now the ball, B, be brought close to the prime conductor, whilst the machine is in good action, a beautiful luminous electro-sphere will be seen covering the lower side of the ball, *b*, but no light on *c*. If a moveable pump-plate be employed, and the whole removed from the pump, the instrument may be held in the hand by the brass cap on the top, and the plate brought close to the prime conductor. The lower ball, *c*, will then have its upper surface adorned with the luminous electro-sphere, and no light will be seen on *b*. This experiment was first described by Father Beccaria, in his *Treatise of Electricity*, published at Turin, in 1753.

I have frequently repeated this beautiful experiment, and have found that the luminous electro-sphere can be exhibited without the aid of an air pump, even in the open air. Considering, as others have done, that the pressure of the air is one of the principal causes of the electric matter being kept either within or close to the surface of charged conductors, it appeared likely that a partial removal of that pressure was the reason of the appearance of that matter in Beccaria's experiment; and if so, why not its appearance in the open air with a stronger electric charge in the conductor? To ascertain how far this reasoning would be sanctioned by experiment, I put my ten-inch cylindrical machine in excellent order, and made the room completely dark.

The machine was kept in vigorous action by my assistant, but it was not until after some considerable time had elapsed that I saw the electro-sphere on the ball of the prime conductor. This occurred after a great number of fine sparks had been taken from the ball—a process which I have since found conduces to the exhibition of the phenomenon, though by no means essential to its appearance.

Fig. 2 will serve to represent the remote extremity of the prime conductor and its ball, with the luminous electro-sphere. When the machine is in good action, I never fail to see this light on the ball after an exhibition of the Aurora Borealis experiment, when the receiver is removed from the ball of the conductor. And, on some occasions when no ball has been attached, I have observed a similar light, partly enveloping the most remote convex surface of the prime conductor; though more frequently this light caps a few prominences only, which stand amongst the indentations occasioned by accidental blows, which that extremity of the conductor has received.

produced it was in a very rapid motion. Nay, what observer has not seen lightning traversing a long track of atmosphere ?

24. The mechanical phenomena, producible by Electricity, are so exceedingly numerous and obviously demonstrative of the materiality of their origin, that, independently of any other, they alone afford abundant evidence of the fact. If a hard steel bar be placed vertically, or even horizontally, with its axis in the magnetic meridian, it becomes magnetic by submitting it to a violent electric discharge ; but if the force of the electric discharge be so far abated, by transferring it through inferior conductors as not to produce a sufficient degree of agitation of the steel, no such magnetic effect takes place. These effects are precisely those which would be produced by the blows of a hammer, or by any other mechanical power. Smart blows, sufficient to agitate the steel, gives an opportunity for the earth's Magnetism to polarize the bar ; but when the blows are feeble, no such magnetization is produced.

25. If a discharge be directed through a piece of wood, the latter will be cleft or split into shivers : a piece of soft clay becomes disturbed and hollow in the middle, or shattered to pieces, by a similar process. If the point of a bent wire be placed against the inner surface of a glass phial filled with oil, and sparks be taken at the other end of the wire, the glass soon becomes perforated ; and many perforations may be made in a short time by moving the point to different parts of the glass, and holding the finger opposite to it on the outside. In all these instances, and in many more that might be adduced, we have direct evidence of the operations of a peculiarly active matter, whose powers are still further manifested by its grinding to an impalpable powder the side of a jar, or other piece of glass, through which it has forced its way.

26. Besides the tangible, ocular, and auditory manifestations of the operations of an active subtile species of matter which appears essentially existent in the display of electrical phenomena, the olfactory and palatic organs also bear testimony of its peculiar impressions. Every Electrician is perfectly familiar with the remarkable odour developed by a machine in good action ; and the peculiar tartness produced in the mouth by the application of two morsels of connected metal to the tongue, or the polar wires of a Voltaic battery to the opposite sides of the face, is also well known to most persons accustomed to the use of these apparatus.

27. The former effect is producible in a variety of ways, by some of which it may be retained for a considerable time after the machine has ceased to be in motion. The room in which a machine has been working for some time, will evince electrical excitement, to any one habituated to the specific odour, for even an hour or more after the process has ended. And the Aurora Borealis experiment never fails to leave a strong electric odour in the receiver for a long time after it is taken from the pump plate. This latter fact is the more remarkable and important because of the odour being produced in the vessel when nearly exhausted of atmospheric air ; and appears to mili-

tate against the idea of its production from secondary causes, especially from those chemical changes which might have been supposed to have taken place in the atmospheric air, unless it can be proved that such changes are more easily accomplished when that air is much atenuated from the natural standard of density at the earth's surface. But even admitting that the smell and taste so eminently distinguishable by electric action are secondary effects, their testimony of the entity of a primitive electric agent would be no less manifest; because the chemical changes themselves would be referrible to that agent, since neither the elements of the air nor of the saliva evinced the least tendency to such change, either prior or subsequent to the electric process. Hence we discover that every organ of sense is more or less, directly or indirectly, susceptible of impressions from electrical phenomena, which transmit to the mind those special kinds of intelligence for which they are respectively and appropriately adapted; and it is by the intelligence which these impressions communicate, and by these alone, that our ideas are to be formed, our reasoning regulated, and our decisions ultimately arrived at respecting the entity or nonentity of an electric agent. Moreover, from the impressive evidence derivable from this source of intelligence of the existence of an electric agent, and the total absence of facts, or even strict analogies, from which inferences could be drawn to the contrary, we are constrained to acknowledge the entity of this agent, and to abandon the idea of accommodating electric phenomena to the indiscriminate, and hitherto undescribed motions of those bodies on which they are usually exhibited.

28. To enumerate all the facts which manifest the existence of a peculiar matter from which electrical phenomena emanate, would be an unnecessary labour for the present purpose: they would require a volume for their description, and much time for their arrangement and explanation. I have brought forward those only which appear most conspicuous to common observation, and perhaps best known to the greater part of those persons in whose hands this Memoir is likely to be placed; and, at the same time, sufficiently obvious to be understood even by those with only a moderate degree of electrical knowledge. Moreover, as I am perfectly familiar with every fact that I have adduced, and contemplated them with much care, I labour under no apprehensions of being suspected of entire ignorance of my subject; and have no hesitation whatever in submitting this investigation to the candid scrutiny of the ablest Electrician. In every part of it there has appeared to me full and unequivocal evidence of the existence of an electric matter, and which I have been led to believe is perfectly distinct from all others, even the *magnetic* and *calorific* not excepted.

AN EXAMINATION OF VARIOUS THEORIES ON ELECTRICITY.*

TWENTY-SECOND MEMOIR.

PART II.

(Read February 3d, 1838.)

Historical Evidence in favour of the inferences drawn in the preceding Part.—Electric Fluid—Du Fay's opinion of two Electric Fluids—Watson and Franklin's idea of one Electric Fluid, sui generis—The author's Theoretical Views—Electrical Attraction—Electrical Repulsion, and its Analogies in Physical Science—Electro-Polarization by locality—Various Explanations of this Phenomenon—Lord Stanhope's Electroscopic Experiments examined.

29. The first part of this Memoir has already been read before the Electrical Society, and is solely devoted to an inquiry into one of the fundamental elements of the theory of Electricity ; for, being the commencement of a series of Memoirs which I have undertaken to bring forward, for the purpose of conveying a comprehensive view of the various classes of electrical phenomena, and of the laws which appear to give them existence, it was deemed necessary, in the first place, to become perfectly satisfied respecting one grand theoretical particular on which much subordinate matter seems mainly to rest, but, concerning which, a very great difference of opinion has latterly been entertained by philosophers of the first degree of eminence in this branch of physics.

30. The grand controversial point, or theoretical question, will be the most intelligibly enunciated in the following manner:—Are electrical phenomena traceable to the operations of a material elementary agent, peculiar in its character, and distinct from every other species of matter? Or can those phenomena be more easily accounted for independently of the operation of such an agent?

31. It is somewhat singular that, notwithstanding all the evidence of the best Electricians that the world ever produced in favour of the entity of an electric matter, an opposite opinion should *now* be started without the slightest foundation for its support, or even one single fact in its favour. For my own part, I have so long been convinced of its existence, and founded my reasonings so completely on its operations, that nothing but a profound respect for the candour and intelligence of some of those

who have speculated on the novel hypothesis, and expressed their proneness to dispense with the electric matter; and a particular desire to place before this infant society the principal facts which bear on this point, in a compact and undisguised form, that I could have been induced to devote the time necessary for their re-investigation, arrangement, and adaptation to this topic. Under these considerations, however, I have undertaken the re-perusal of many authors, and have read some others to which I had before not paid sufficient attention to form an opinion of their sentiments, and the value of their personal labours in this department of science. And I have also been induced to repeat several old experiments, and institute some new ones, in order to satisfy myself in certain particulars, and make myself perfectly acquainted with every fact on which my reasonings have been founded. The first part of this Memoir is principally devoted to these investigations, and the conclusions I have there arrived at are the natural inferences derivable from the various circumstances connected with the phenomena already explained. I have also extended my inquiries to other electrical phenomena, both mechanical, physiological, magnetic, thermometric, and chemical; and, from a rigid examination of them, and a comparison of the various ways by which some of them have been attempted to be explained, I can discover no hypothesis so free from ambiguity, none so truly specific, none so simple, distinct, and comprehensive—in short, none so apparently rational and conclusive as that which admits of the agency of a purely electric matter.

32. The electric matter was recognised in a very early period of the progress of the science by some of the most active and discerning philosophers of that day. Benjamin Wilson, than whom no one was more conversant with all the then known phenomena of Electricity, did not hesitate to attribute their exhibition to the operations of an electric matter, which he was disposed to assimilate to the ether of Sir Isaac Newton; although he gives it the peculiar appellation *electric matter*.* Stephen Grey, who made many capital discoveries in Electricity, calls this matter the "electric fire,"† which is an appellation given to it by Father Beccaria;‡ and was also employed by Dr. Watson, who says, "that the electrical fire is truly flame, and that extremely subtile."§ Many other philosophers of considerable eminence have given to the electric agent the name of electric fire—a term frequently used even at the present day; although "electric fluid" has, in a great measure, superseded it. Viscount Mahon, afterwards Lord Stanhope, called it the "electric fluid;"‖ and Priestly also uses the same term for the electric agent.¶ Euler appears not so partial to an "electric fluid,"

* Wilson's Treatise on Electricity, 1750.

† Philosophical Transactions, original No. 436, page 16; or Hutton's Abridgement, vol. viii., page 5.

‡ Beccaria's Treatise upon Artificial Electricity, Translation 1776.

§ Philosophical Transactions, original No. 471, page 481. Hutton's Abridgement, vol. ix., page 158.

‖ Principles of Electricity, by Charles Viscount Mahon, 1779.

¶ Priestley's History of Electricity.

although he frequently employs the terms *positive* and *negative* electricity ; and undertakes to explain the whole phenomena, by the operations of an highly elastic *ether*, with which the pores of all bodies are continually charged. This ether, being susceptible of compression and dilatation, would assume different degrees of density accordingly as the pores of bodies were closed or expanded ; which was the cause of those bodies becoming *positively* or *negatively* electrical respectively. Euler admits of the transmission of this *ether* from one body to another, and explains the spark, shock, lightning, and thunder in precisely the same manner as other philosophers have done with an " electric fluid," differing from them only in the name of the electric agent.* Cavallo was exceedingly cautious in adopting any hypothesis for the explanation of electrical phenomena ; yet, with all his diffidence, he very frankly confesses that the supposition of a purely electric fluid is certainly the most probable.† And in another place, when speaking of the *residence* of this agent, Cavallo says, " that the electric fluid, proper to a body when in its natural state, is equally diffused throughout all its substance, *I think no one will deny* ;"‡ which expresses this philosopher's conviction not only of the existence of an electric matter, but also of the mode of its distribution. The experiments of Du Fay led that philosopher to the belief of the existence of *two* electric fluids, independently of which he could not account for the phenomena which they presented to his notice. The hypothesis of Du Fay has been very much esteemed by the continental philosophers, and by some of them is still held in considerable repute.

33. In an early part of the year 1747, Dr. Watson made known his ideas of the operation of *one* electric fluid *sui generis* ;§ and about the same time, in America, Dr. Franklin was digesting his well-known theory, which also rests upon the supposition of one electric fluid.‖

34. The theory of Franklin, so far as it extends, appears to require but very little modification to become applicable to every fact that has been developed in this branch of physics prior to the discovery of Electro-Magnetism ; and perhaps it is in this department of Electricity alone where the Franklinean doctrine will be found materially deficient. And even here, notwithstanding its inadequacy to account for this class of phenomena, it does not appear to be materially defective in itself, or physically incorrect ; for the principles of that doctrine are as decidedly and as conspicuously in operation in this department of Electricity as in any other ; and, by annexing the principles of Electro-Magnetism and Magnetic Electricity to those which Franklin had embraced in his theory, it is probable that we should be in possession of a code of laws, to the operation of which every known phenomenon may be traced.

* Euler's Letters on different subjects in Natural Philosophy, addressed to a German Princess. Brewster's Translation, vol. ii.

† Cavallo's complete Treatise on Electricity, second edition, pages 105-114.

‡ Ibid, page 136.

§ Priestley's History of Electricity, page 380.

‖ Ibid.

35. The same mathematical formulæ are as easily deducible from the vitreous and resinous forces of Du Fay as from the positive and negative forces of Franklin; and consequently as applicable to the doctrine of two electric fluids as to that of one electric fluid. But it cannot be said that, because mathematical processes are rigidly correct, that such circumstance confers on them the attribute of infallibility in testing the correctness or incorrectness of our notions respecting the physical agency which actuates in the production of natural scientific events. In the instance before us, we have a pretty fair specimen of the *flexibility* of mathematics, which are obviously as applicable to false as to true data; for it would be an absurdity to suppose that both these theories are physically correct. The fault, however, is not in the mathematics, but in the data on which the reasoning is founded. The data once given, a moderate degree of skill would enable the mathematician to proceed in his investigation and arrive at results perfectly agreeable to the data; but should the latter be incorrect, the exactness of the investigation could have no tendency whatever to establish a true theory; for the foundation, resting on no physical truth, the superstructure itself, in whatever manner it might be adorned with mathematical symbols, would be too frail to withstand those stern facts which the flexible laws of nature develop to rigorous experimental research.

36. Mathematical investigations, however, when directed to physical operations, and based on incontrovertible facts, give a sterling value to science, and establish imperishable sources of conduciveness to the various wants of civilized life.

37. Mechanics, Hydrostatics, and Pneumatics are amongst those experimental sciences whose phenomena are calculable with mathematical precision; and which, in consequence, have long become practically and extensively useful. The phenomena of those sciences requiring no dexterous hand for their exhibition, nor presenting any equivocation or capriciousness in their display, were easily reconciled to that degree of certainty and exactness which alone render experimental results susceptible of utile mathematical rule.

38. In Electricity, however, the phenomena are of a very different character, presenting difficulties not to be met with in any other branch of physical research. The number of electrical phenomena is so exceedingly great as to surpass the collective sum of all others that science has revealed to man. They are exhibited under such a variety of circumstances, both on masses and on the most minute portions of matter, are brought into play by such a diversity of both natural and artificial means, and viewed by experimentalists under such a dissimilarity of aspects, that it is scarcely to be wondered at that the multitude of facts which have been developed in this branch of physics remain to this day little better than a stupendous heap of inorganized materials—intrinsically rich, but whose real value can never be justly appreciated until they have obtained a natural and obvious classification, and are reduced to some

certain inflexible laws, by which they may assume not the habiliments only, but the real character of genuine systematic science.

39. From this view of the present condition of Electricity, it is obvious that much still remains to be done before this branch of physics becomes sufficiently matured so as to be established within the precincts of exact science; and, perhaps, it is only from the repeated efforts of close and exact observers, who are well accustomed to the practical part of Electricity, that much progress is to be expected in systemizing the phenomena to uniform scientific order. And perhaps, also, to attempt anything further than a mere step in the advancement of Electricity would be more than could reasonably be expected to be accomplished by any individual in the present disorderly condition of the materials which are placed before him, all of which demand attention, and require the strict scrutiny of the most acute and profound Electrician. I do not, therefore, in this investigation entertain too sanguine a hope of arriving at satisfactory conclusions on every point which I have to discuss, though I hope to be enabled to succeed in explaining some particulars which still remain subjects of controversy and dispute. Moreover, as in this series of Memoirs I shall have to notice some tributary investigations in which interesting facts have been elicited, whose explanation seems to rest on those very points about which philosophers are still at issue, it becomes essential that I make known, as early as possible, those principles upon which my theoretical reasonings are intended mainly to rest.

40. The theoretical views which I entertain of common Electricity are, perhaps, not very different to those embraced in the Franklinean doctrine. I attribute electric phenomena to the agency of a peculiar species of matter, or electric fluid, whose particles mutually repel each other, but which are attractive of all other kinds of matter. By virtue of the innate repulsiveness of its particles, the electric fluid becomes highly elastic, and is compressible and dilateable by the application or removal of external forces. The dilateable propensity of the electric fluid gives it a tendency to spring outwards with an equable force in every direction as from a centre, which force is proportional to the force of compression, or to the density of the fluid. The electric fluid is transferable from one body to another; and, like all gaseous fluids, flows with the greatest facility in the line of least resistance.

41. The atmosphere, as far as has been explored, is continually charged with electric fluid, and in a greater degree as we ascend from the earth's surface (13 and note). From this condition of the atmosphere, it is obvious that all bodies on the earth's surface are subjected to an electric pressure as decidedly as a solid ball of matter would be subjected to an elastic pressure if suspended in an atmosphere of gas. By this electric pressure the fluid forces itself into the pores of all bodies in proportion as they offer facilities for its admission; and as different bodies offer different degrees of

facility, they necessarily become charged with it to different degrees of extent. The attractive quality also, differing amongst those bodies, will also be another means of their being differently electrized under the ordinary pressure.

42. Moreover, as bodies generally, in their natural condition, are compounds and not elementary, it follows that the particles of those bodies which are of a different elementary character are naturally in a different electric condition. Hence heterogeneous bodies, however compact they may appear to common observation, are not uniformly electrized; every particle of one of the constituents being in a different electric state to every other constituent element in the compound.

43. The natural electrization of bodies is still further diversified by their mechanical structure and external polish; for a difference in the compactness of one and the same kind of matter confers on it a difference of capacity for the reception of the electric fluid. And again, the same kind of metal, though of the same compactness throughout, will have a different electric capacity by being of different degrees of polish on different parts of its surface.* Hence, by having its opposite surfaces of different degrees of polish, those surfaces will be of different capacities for the reception of the electric fluid, and will, under the common pressure, be positive and negative with regard to each other (13).† Hence it becomes obvious that unless a body be perfectly homogeneous in itself, and of equal polish on every part of the surface, it is physically impossible that it can be equably electrized throughout, or even of uniform electric tension on every part of its surface.

44. The electric conditions of bodies in all the cases above mentioned (41, 42, 43) are those which they would assume under the ordinary natural electric pressure, when equably distributed on every side alike; or whilst they are perfectly surrounded by the atmospheric air, and sufficiently distant from other bodies not to be influenced by the fluid they contain; for when one body is in contact with another body, since their natural electrical conditions depend upon a circumambiency of equable pressure—that equable exposure being destroyed by the plane of contact—the bodies change their electrical character, and a new equilibrium is formed, different to that which either of them assumed separately.‡ Hence it follows, that as the materials composing the earth lean against, or rest upon, one another, the individual masses are seldom, if ever, exposed to an equal circumambient electric pressure, and consequently are in electric conditions accordingly to their natural dispositions (41, 42, 43), conjointly with their connections with one another.

* See Eighth Memoirpage, 201. † Ibib.

‡ There is no experiment more decisive on this point than that first shown by Volta with the copper and zinc discs. Before the discs have been brought into contact with each other, each metal has its share of electric fluid due to an equable circumambient electric pressure; but, when their faces are in contact, the natural pressure is removed from the joining surfaces, and a portion of fluid flows from the copper to the zinc, or in the line of least resistance (40), both metals assuming thereby new electrical conditions.

45. From these considerations we are led to understand that all compound particles are electro-polar, or have their opposite sides in different electrical conditions (42); and this will be the case, whether the compound be solid, liquid, or gaseous. We learn also that bodies even of the same kind (instance pieces of metal), become electro-polar from a variety of causes (13, 43, 44). Bodies also become electro-polar by an unequable pressure on their opposite surfaces, though not in contact with other bodies. This kind of electro-polarity arises from a local disturbance of the natural equilibrium by a vicinal superior or inferior electric force, located on one side only, or by the influence of both forces on opposite sides at the same time (40, 52, 54).

46. As bodies, when in contact with each other, assume an electric equilibrium as decidedly as if perfectly insulated (44), it follows that the bodies composing the earth's surface, and the circumambient atmosphere, would constantly observe an unvarying electric equilibrium, were no physical changes to take place amongst them. But as the whole are continually undergoing a change of temperature, corresponding electrical changes are as constantly going on, not only from the *direct* thermometric variations, but *indirectly* from a variety of secondary events, or physical vicissitudes in the liquid and aerial matter ; by means of which the electric fluid is scarcely ever at rest, being transported from one body to another as their capacities for its reception alter, and giving rise to other phenomena, and various modifications of matter, unproducible by any other natural agent.

47. Notwithstanding the various degrees of susceptibility which bodies present for the reception of the electric fluid, its natural elasticity, or innate repulsiveness, appears to be essential to its intromission ; unless the mutual attractions between it and other matter be regarded as the only necessary qualification for its universal presence throughout nature, and its surprising volant motions through the air, and active transiliencies among the generality of terrestrial bodies.

48. Electrical attraction is an attribute well known from the earliest period of the science, and has maintained its position in almost every hypotheses hitherto framed ; and appears to be sufficiently obvious and self evident in the display of some electrical phenomena as to be understood by every observer. The opposite force, however, has had to encounter many theoretical difficulties which some philosophers have shown a proneness to place in its way; and is far from being generally acknowledged, as an existing principle, at the present time. Indeed, electric repulsion is totally discarded by some writers, although this force seems more essentially concerned in the production of the generality of electrical phenomena than even attraction itself. And as some of those phenomena appear perfectly inexplicable by the attribute of attraction alone, I have not hesitated to introduce *repulsion*, as a fundamental principle, into the groundwork of the theory I have advanced, considering it as a natural attribute of the electric fluid (40); and I hope to be enabled to show, not only its efficacy in the pro-

duction of phenomena, but its absolute indispensableness in the explanation of some of them.

49. The repulsion attributable to Electricity has many analogies in physical science. No philosopher has yet disputed the existence of magnetic repulsion, not even those who have denied the existence of a similar force in Electricity, although, in many instances, the experimental evidence is as favourable in the latter as in the former class of phenomena.

50. Elasticity is a fundamental principle in the study of Pneumatics universally acknowledged without even a dissentient opinion. But to what attribute of its particles is the elasticity of the air traceable? Why does the remaining air, in a receiver, attenuate *itself*, and fill the whole capacity after a portion has been withdrawn by the action of the pump? And why this attenuation and expansion of even the last remaining portion of air, after the most perfect exhaustion that the pump is capable of making? These are important questions to which the term "elasticity" gives no satisfactory answer. "Elasticity is expressive of no cause, signifying only the capabilities and susceptibilities of the body to which, as a convenient term, it is applied. Elasticity depends upon an innate repulsive power, which the particles of air, or other gaseous media, naturally possess, and which is indispensably essential to their existence as elements of elastic fluids. Repulsion is, therefore, an atomic attribute, and the most remote physical cause to which the expansive force of air can be traced; whilst elasticity expresses both the tendency to expansion, and the susceptibility of compression, which extensive groups of the repulsive particles exhibit.

51. Now most writers on Electricity have defined the electric fluid as highly elastic; and as we have seen that an innate repulsiveness of the elementary particles is obviously the primary physical cause of all fluid elasticity, this natural attribute is as applicable to the electric fluid as to the atmos-fluids. Hence, as far as elasticity is concerned, there is no more impropriety in attributing the cause to an inherent atomic repulsion in the one case than in the other. And, although we do not find condensed portions of atmos-fluids repelling one another without any apparent connection, in the manner we observe repulsions amongst positively electrized bodies, we have ample analogy in Magnetism (49); and perhaps we have no reason to expect those analogies to hold good in the infinitely grosser and comparatively inactive atmos-fluid matter.

52. I know of no fact in which electric repulsion is more obviously the cause than the polarization of bodies by locality (45), although there be an abundance of them which bear ample testimony of its essential influence. If a positively electrized body, P, Fig. 3, Plate XV. be brought to within a short distance of an insulated body, B, it is well known that the latter will become electro-polar; being negative at the extremity, *n*, but positive at P, and neutral at about the central section, C B. Now I would ask any philosopher who has contemplated this beautiful and highly interesting fact

with that degree of attention which it so eminently deserves, to what power he would ascribe its appearance? Certainly not to any attractions that could possibly be devised. The attraction of the body for the electric fluid at *n* could not be lessened, nor that at P augmented by the approach of the body P. We do not attribute the electrical appearances to any change that has taken place in the metal, but a *disturbance* of the electric fluid which it contains, or which resides within it or about it. But a disturbance implies a disturbing force, which force must have been in or about the body, P, because the phenomenon was not exhibited prior to the location of that body, nor does it continue only during the locality of the bodies, P and B. Moreover, the mere metal of the body, P, did not constitute the disturbing force, because, if that body had been in its natural electrical condition, the polarity of B would not have happened. Hence the disturbing force proceeded from some natural attribute of the electric fluid in or about the body, P, and not from any property of the metal itself. Now, an attraction between the fluid in P and the metal of B could be no means of the latter becoming electro-polar in the manner shown by the experiment; nor can it be shown that such attraction would cause any disturbance of the fluid in B, unless there were an absolute introgression of fluid to B—a circumstance neither known nor supposed to take place; for when P is removed to a sufficient distance, the body, B, is found in its natural inert electrical condition.

53. The difficulties presented to the explanation of this species of polarity, by rejecting electrical repulsion, are entirely removed by the admission of that principle or attribute into the theory. Let the accumulated fluid on the body, P, repel the fluid in the body, B, and the explanation becomes exceedingly easy and familiar. The body, B, being a good conductor, its fluid would be easily put into motion by the disturbing repulsive force on P. By this repulsive force the fluid originally occupying the extremity, *n*, of the body, B, would be partially dislodged, and driven towards the extremity, P. The extremities, *n* and P, would then be respectively negative and positive, as shown by the experiment.

54. The distribution of the fluid on B will vary accordingly to the extent of disturbance; which may be made to vary either by varying the electric power of P, or by varying the distance between the two bodies. Hence, it is obvious that the neutral plane, B C, will change its position accordingly with these circumstances. The neutral part of B can hardly be defined as a plane in all cases; because, when the conductor, B, is large, there is a neutral zone of some considerable dimensions whose position will vary with the circumstances connected with the disturbance.

55. If, instead of being located with the positively electrized body, P, the conductor, B, were in the vicinity of a negatively electrized body, N, Fig. 4, the character of polarization in B would be the reverse of that in the former case; or the nearest extremity, P, would be positively, and the remote extremity, *n*, negatively electrical. To explain

this event, we have only to understand that the natural electric pressure (41) on the extremity, P, is lessened by the presence of the negative body. The fluid in B will now obey the law observable in all fluids, and will move in the line of least resistance, or towards the extremity, P; and thus the body, B, will become electro-polar. When the negative body, N, is sufficiently removed, the fluid of the body, B, will again experience an equable circumambient pressure (41), and will consequently resume its former natural distribution.

56. The experiment which I have been describing has been known to Electricians for many years,* and the explanation which I have given of it is far from being new. It is in principle the same as is given by several authors, but perhaps the most minutely described by Viscount Mahon, afterwards Lord Stanhope, by whom the greatest variety of experiments were made on loco-electrization that are on record.† I have repeated most of this Nobleman's experiments on loco-electrization, and have found his descriptions of the phenomena exceedingly correct, although in some others he appears to have been very much deceived. Beccaria also made many interesting inquiries in this class of phenomena,‡ which I shall have more particularly to notice in another place. Dr. Milner, of Maidstone, also made many exceedingly interesting experiments in loco-electrization, which were diversified in a great variety of ways.§ I am well aware that Lord Stanhope's explanation of these phenomena has not been generally received as orthodox Electricity, although it is by far the simplest and most intelligible that has been proposed, and in every other attempt at explanation the principle of *repulsion* is obviously resorted to.

57. Mr. Morgan has attempted to repudiate Viscount Mahon's theory with greater determination than perhaps any other writer on this subject; but his views of different phenomena are not very consistent with each other, and, with reference to the present question, not very satisfactory. Whilst speaking of the polarized body, Mr. Morgan says, in rather a lofty tone, "a metallic body is said to be in two different states at the same time. What single electrial fact is to warrant this assertion? What is there intelligible in Electricity, if we admit that perfect conductors can have there equilibrium disturbed, or that two different parts of them can be at the same time in two different states, when there is no kind of insulation to separate the positive from

* Otto Guericke, a famous philosopher and Burgomaster of Magdeburg, who flourished about 1670, was the first to discover that bodies could be electrized without having the fluid communicated to them.—(Priestley's History of Electricity, page 8.) Some experiments by Stephen Gray also showed electrization by locality. This ingenious Electrician, in the year 1730, electrized a boy, suspended by silken cords, by bringing near to him an excited glass tube. When the tube was brought near to his feet the greatest action was about his head.—(Philosophical Transactions, original No, 418, page 81. Hutton's Abridgment, vol. vii. page 489. Priestley's History of Electricity, page 32.) But Canton, Franklin, Æpinus, and Wilcke, made the first interesting series of experiments on this branch of Electricity.—(Priestley's History of Electricity, page 211.)

† Principles of Electricity, by Viscount Mahon, quarto, 1779.

‡ Giambatista Baccaria dell' Electricismo Artificiale e Naturale, 1753, Turin, quarto. English Translation, sec. 3, 1776.

§ Experiments and Observations iu Electricity, by Thomas Milner, M.D. 1783.

the negative, but, on the contrary, such a communication as in every other instance immediately restores the equilibrium?"* Perhaps, whilst Mr. Morgan was writing this sentence, he had forgotten that he had previously shown that a continuous conductor could be both positive and negative at the same time. Whilst describing the electrical conditions of the coatings of one jar and the linings of another, in metallic connection with each other, he says, "the deficiency is in connection with the superabundance."†

58. Those Electricians who have written in favour of Du Fay's hypothesis of vitreous and resinous fluids, explain the phenomenon (52) upon the principle of decomposition. The vitreous fluid of the body, P, Fig. 3. attracts the resinous fluid of the conductor, B, and at the same time *repels* the vitreous fluid of B to the most remote extremity, P. When the conductor, B, is located with a negatively electrized body, N, Fig. 4 (55), the resinous fluid of the body, N, is supposed to attract the vitreous fluid of the conductor, B, and draw it towards the nearest extremity, P; whilst, at the same time, the resinous fluid of N repels the resinous fluid of the conductor, B, to the remote extremity, *n*. Thus, in both cases, *repulsion* is required to explain the phenomena. The same principles are here applied to *both* fluids as Franklin gave to his *one* fluid; and as by the latter the problem is easily solved, there can be no philosophical propriety in admitting the compound fluid of Du Fay.

59. The phenomenon which appears to present the greatest impediment to the universal reception of electrical repulsion is the separation of light bodies when negatively electrized. The hypothesis of Du Fay, admitting of electric repulsion in both its fluids, is perfectly prepared for the explanation of this phenomenon, by alluding it to the natural attribute common to the vitreous and resinous fluids, which, although attractive of each other, are individually repulsive of themselves (58). Hence the difficulty in solving the problem which this phenomenon presents is limited to those who admit of one electric fluid only; some of whom have had recourse to the supposition that all matter is repulsive of itself, and that bodies which are negatively electric exhibit this supposed universal attribute more perfectly than when in any other condition. This method of explaining the phenomenon was first proposed by M. Æpinus,‡ and afterwards adopted by Mr. Wilcke,§ and still further supported by the Honourable Mr. Cavendish, who made an immense number of experiments, with some mathematical investigations, which to him appeared favourable to that hypothesis.|| The same views of negatively electrized bodies are still favoured by some more modern writers on this subject.

* Morgan's Lectures on Electricity, vol. ii. page 275, 1794. † Ibid, vol. i. page 110.

‡ Tentamen Theoriæ Electricitatis et Magnetismi, Petersburg, 1759. Priestley's History of Electricity, page 395.

§ Ibid, page 396.

|| Philosophical Transactions, Hutton's Abridgement, vol. xiii. page 223.

60. To me there appears no necessity to load the theory of Electricity with this auxiliary force, because other methods of solving the problem are quite as satisfactory. Let the two balls, x y, Fig. 5, be attached to a negatively electrized body, B. Now the balls being deprived of their natural share of fluid, will endeavour to recover it again from the nearest portions of the surrounding air, and in consequence of both of them drawing a supply, at the same time, from that plate of air directly between them, it will become more negative than any other stratum in the vicinity of the balls, and consequently less enabled to continue the supply. This being accomplished, and the balls giving their newly acquired fluid to the body, B, as fast as they collect it, they will remain negatively electric. There will now be an attraction exercised between these balls and the electric fluid of the atmosphere; and as the balls yield this fluid to the body, B, more freely than it is capable of extricating itself from the distant particles of the air, the balls travel in quest of new supplies towards those places where it is most abundant, or from the stratum which they have already partially deprived of its electric fluid, Hence it is that the balls move in opposite directions from their original plane of contact, and by their divergency appear to repel one another, although it is probable that the phenomenon does not essentially depend upon repulsion, but is principally the consequence of electrical attractions.

61. Some experiments related by Earl Stanhope have been the cause of more theoretical speculation on this particular topic than any other with which I am acquainted, and, like some of more modern date, have been productive of very much delusion. The experiments, and the inferences drawn from them, are described in the following manner:—

" *a. Experiment* 1, *Fig.* 6.—I took a pair of cork-ball electrometers, *a b*, whose balls were three-eighths of an inch diameter, and whose parallel legs were eight inches long; and I suspended them to a hook that was fixed to the underside of the brass cap, C D, of the glass receiver, E, F, G, H, of an air-pump, as shown in the figure. The two legs of this electrometer, in order to prevent their twisting, were made of fine straws which had been previously well soaked in salt and water, to make them conduct better. Each of these legs was suspended at top by a very fine linen thread of about *one-twelfth* of an inch in length.

" *b.* In order to render the glass receiver a good non-conductor of Electricity, I caused it to be perfectly well dried by means of fire. It was proper, in this experiment, to avoid giving any friction to the glass receiver for fear of charging the glass.

" *c.* Upon charging a small prime conductor, which was made to communicate with the brass cap of the glass receiver, the electrometrical balls divaricated above *two inches and a half.*

"*d. Experiment* 2.—I then began to *exhaust* the receiver, upon doing which the balls soon began to divaricate less and less. And as soon as the short barometer gage was got

down to about *one-quarter* of an inch (the barometer being that day at the height of *twenty-nine inches and a quarter*), the divarication of the balls from each other become reduced to *less* than *one-quarter* of an inch.

" *e.* So that by $\frac{116}{117}$ parts of the natural quantity of air contained in the receiver being *exhausted*, the divarication of the electrometrical balls was diminished to less than *one-tenth* part; for the *chord* of the angle of divarication was decreased (as was said before, *c d*) from above *two inches and a half* to less than *one-quarter* of an inch—that is to say, that the *versed sine* of the angle of divarication, was decreased considerably more than an *hundred times*, because the *versed sines are always as the square of the chords.*

" *f.* I should be inclined to imagine, if this experiment were made with great accuracy, and with a proper electro-meter, that the *versed sine* of the angle of divarication would always be in the *same ratio* as the density of air in the receiver, provided that proper means were taken to keep the apparatus sufficiently free from moisture during the experiment.

" *g. Experiment* 3.—I then electrified the glass of the receiver itself; but as long as the receiver remained *exhausted* to the degree above-mentioned (*d*), it was out of my power to cause the electro metrical balls to divaricate above *one-quarter* of an inch.

" *h. Experiment* 4.—I then let the air return into the receiver, which circumstance alone caused the balls again to divaricate considerably, although I gave to the apparatus *no fresh supply of Electricity;* but the balls took up a short time in coming to their full degree of divarication—the reason of which evidently was, that the *unelectrified air* which entered into the receiver could not receive *immediately*, from the charged apparatus, that degree of Electricity which it was able finally to acquire.

" *i.* I repeated these experiments with the exhausting air pump several times, and I always found results that were similar.

" *j.* From these experiments it appears that, when bodies are charged with Electricity, it is the *particles of (circumambient) air being electrified* that constitutes the *electric atmosphere* which exists around those bodies.

" *k.* Now, since an *electrical atmosphere* (whether *negative* or *positive*) consists of electrified air, it evidently follows that the density of the Electricity of the air must be in some *inverse ratio of the distance* from the charged body which causes that electric atmosphere.

" *l.* That electric atmospheres do decrease *in density*, the more the distance from the electrified body is increased, is demonstrable by means of a proper electrometer in every instance.

" *m.* From these simple considerations, it is easy to reduce all the different *phenomena* of electrical *attraction* and *repulsion* to *one* plain and convenient principle, derived from the very nature of a *disturbed electrical equilibrium;* namely, to the elastic

tendency of the electric fluid to impel every body, charged either *in plus* or *in minus*, towards *that part* of its electric atmosphere where its *natural electrical equilibrium* would be the most easily restored.

" *n*. From this simple principle, it is evident that bodies which are charged with contrary electricities must tend to *approach* each other whenever the skirts of their (oppositely electrified) *atmospheres* interfere.

" *o*. From the same simple principle, it is also easy to understand why bodies that are charged with the *same kind* of Electricity tend to diverge from each other.

" Every body that is electrified (whether *in plus* or *in minus*) has a constant tendency to return to its *natural state ;* and this causes it to electrify, in a certain degree, *other bodies* in contact with it, and the *air* in its vicinity, in a manner similar to that explained above.

" *p*. If two bodies, for example, be both *positive*, neither body will be able to deposit its *superabundant* Electricity upon the other body, which is also similarly electrified *in plus*. It is therefore evident, from the simple principle mentioned above (*m*), that if these bodies be brought near each other, *each body* will be impelled towards the particles of air on its *other side*, which are electrified *in plus* only in a *small* degree—that is to say, that each body will tend to diverge from the other.

" *q*. If these bodies, on the contrary, be both *negative*, neither body will be able to have its *deficient* Electricity supplied from the other body, which is also similarly electrified in *minus*. It is therefore evident, from the simple principle mentioned above (*m*), that if these bodies be brought near each other, *each body* will be impelled towards the particles of air on its *other* side, which are electrified *in minus* only in a *small* degree—that is to say, that each body will tend to *diverge* from the other.

" *r*. So that bodies which are charged with the *same kind* of Electricity (whether *positive* or *negative*) must necessarily tend to *diverge* from each other."

62. The inferences which the noble author has drawn from his experiments are obviously at variance with the doctrine of electrical repulsion; which is the more remarkable, because he has acknowledged the *elasticity* of the electric fluid (*m*), a property evidently traceable to the attribute of repulsion exercised by its individual particles (50, 51).

63. There can be no objections to Lord Stanhope's method of explaining the phenomena as far as it proceeds, because attraction is unquestionably in play in the divergency of similarly electrized bodies as that phenomenon is usually displayed; but neither the experiments of that nobleman, nor the inferences he has drawn from them, have any tendency whatever to disprove the existence of a repulsive electric force.

64. I am well aware that these experiments are usually looked upon as masterpieces of their kind, and are much admired and frequently quoted by those philoso-

phers whose opinions are hostile to the doctrine of electrical repulsion; and as their correctness has never yet been disputed, they are regarded as affording *standard* data, on which much theoretical speculation has been founded.

65. To me, however, these experiments have never appeared in that light; but, on the contrary, I have always considered the data which they afford much too scanty, if even the recorded results had been admissible as facts on which implicit confidence could have been placed. And, on looking at the circumstances connected with the experiments, it is not difficult to perceive that those results are placed in a very questionable posture; and are obviously objectionable in whatever point of view the scientific Electrician may contemplate them.

66. The electrometer (60 *a*) which Lord Stanhope employed in these experiments was not much calculated to give very exact results in an atmosphere so far attenuated as that it is said to be placed in (60 *d*). Its balls were far too heavy (60 *a*) to be kept divergent by any electric force which they could retain in so good a conducting medium as that of an atmosphere supporting only one quarter of an inch of mercury. The electric force which kept the balls two inches and a quarter apart (60 *c*), in a common atmosphere, would be mostly lost in the attenuated one (60 *d*); for withdrawing the air with the pump would remove the insulation, and consequently a portion of the fluid would make its escape from the straws and their balls, probably to the base of the instrument. But it is stated by Lord Stanhope that the electric force was *not* lost by the attenuation of the air; for he says that, when the air was readmitted, the balls again divaricated "considerably, although I gave to the apparatus *no fresh supply of Electricity*" (60 *h*); and, from the subsequent part of that paragraph, we are led to understand that the balls ultimately diverged to the same extent as at first, a conclusion not very consistent with the doctrine of electric atmospheres (60 *k* to *r*)—for one would be led to suppose that the first electric atmospheres of the balls would be partly removed by the action of the pump, and another pair have to be formed on the readmission of the air, and the formation of these second atmospheres would have to be at the expense of the electric fluid from the balls, straws, and cap of the instrument, in all of which the remaining fluid would be attenuated, and the divergency ought not to be so great as before.

67. I do not know that Lord Stanhope's experiments have ever been repeated; or, if they have, I should suppose, from the estimation in which they are held by some of our latest writers on this subject, that they never have been much varied, and but very imperfectly understood. Singer, who has left the best treatise on Electricity in the English language, speaks of Lord Stanhope's experiments with great confidence, and quotes them in support of his opinion of there being no such attribute as electric repulsion.* Since the publication of Singer's work, I cannot find that any scientific

* Elements of Electricity and Electro-Chemistry, by George John Singer, page 24. London, 1814.

journal has noticed these celebrated experiments farther than an occasional quotation; and therefore I am in hopes that the experiments which I am about to detail will appear interesting, not only to the London Electrical Society, but to philosophers generally who are engaged in theoretical inquiries in this branch of physics.

68. I prepared an electrometer similar to that employed by Lord Stanhope (60 *a*), and proceeded to repeat the experiments in the manner already described (60 *c*, *d*, *h*). Prior to the attenuation of the air in the receiver, the cork balls were caused to diverge to about two inches from each other by the application of an excited glass tube. The pump was immediately brought into requisition, and whilst the air in the receiver was being attenuated, the divergency of the cork balls began to be lessened; and before the attenuated air was counterbalanced by one inch of mercury, the balls got to within a quarter of an inch of each other. The air was now readmitted to the receiver very slowly, but the balls showed no tendency to separate again. I repeated this experiment many times with similar results.

69. It now occurred to me that the cork balls were too heavy, and that, relatively to the two *extreme* conditions of the air (dense and rarefied) in which they were immersed, they would be heavier in one case than the other. This latter circumstance, however, could not prevent the balls from diverging again when the air was readmitted; for if the electric force had continued unimpaired till the return of the air, the divergency ought to have progressed as the density was restored.

70. I now removed the cork balls from the ends of the straws, and replaced them by small balls of the pith of the elder. I again electrized the apparatus with an excited glass tube until the balls diverged to about two inches from each other. On attenuating the air to the same extent as before, the balls approached to within about three-eighths of an inch of each other. The air was readmitted gently, but the balls never separated any farther than whilst the air was attenuated. This experiment I also repeated several times, and the results were always of a similar description, the collapsion of the balls usually bringing them to *within* half an inch from each other when the air to which they were exposed counterbalanced one inch of mercury; but in no case did they diverge again when the air was restored to its usual density. From these facts nothing seemed more likely than that a portion of the electric fluid had escaped by the attenuation of the air in the receiver.

71. My next experiment was intended to ascertain if the pith balls could be made to diverge to two inches in air attenuated so as to counterbalance only one inch of mercury. The air in the receiver having been brought to this standard density, the excited glass tube was brought to the cap of the instrument. The straws and balls now exhibited some strange antic motions not easily described. They would first suddenly diverge to a considerable extent, and as suddenly return to their vertical position in the axis of the glass, repeating these motions two or three times before the

tube came into actual contact with the cap of the electrometer; and it was often with great difficulty that they could be made to remain separated when the excited tube was taken away. From the results of this experiment, often repeated, it seemed obvious that the fluid was given off by the straws and balls either to the sides of the receiver or to the pump-plate. The latter, however, appearing the more probable course for it to take, I contrived the following experiment to ascertain how far this view might be correct.

72. Two electroscopes were provided for this experiment, which, when properly prepared, were placed the one on the other, as represented by Fig. 7. The receiver, A, prior to its being situated on B, as in the figure, was placed on the plate of a jet d'eau experiment apparatus, whilst the latter was screwed into the orifice of the pump-plate. The air in A was then attenuated until it would just counterbalance one inch of mercury. This done, the communication was cut off by turning the stop-cock, *s*; the apparatus was then taken from the pump, and screwed to its base, *d*, and afterwards placed on the electroscope, B, whose contained air was of the density of the atmosphere. On bringing the excited glass tube near to the cap of the electroscope, A, the pith balls made several singular motions, but did not evince much tendency to diverge from each other; but the pith balls within B diverged to an inch and a half at least as decidedly as if the tube had been brought *directly* to its cap. When the excited tube was taken away, the balls in A hung close together, but those in B remained divergent, as shown by the figure. The tube was excited anew, and again applied to the cap of A, whose balls again were much agitated, and the balls within B diverged farther than before, and even struck the sides of the glass. By a few trials I found that any degree of divergency might be given to the balls in B, but that there was great difficulty in keeping those in A separated more than half an inch from each other after the excited tube was taken away; although, by regulating the distance between the tube and the cap of the instrument, they might be made to separate one inch or more.

73. The experiment (72) was repeated by applying a large piece of excited amber, instead of the glass tube, to the cap of the instrument, A. The results were similar to those obtained when the glass tube was employed, only the balls in A were not so much agitated. The balls in B separated as before, though not to so great an extent, whilst those in A separated farther than by the application of the tube, and remained more divergent after the amber had been taken away. The balls in B were made to separate two inches and a half by three applications of the excited amber to the cap of A, and remained separated for some time after the amber was removed.

74. Now although the results of these experiments appeared satisfactory enough that the fluid communicated to the instrument, A, from the glass tube was transmitted through the attenuated air to the lower instrument, B, and when the amber was used

the fluid moved in the opposite direction, yet it was necessary to ascertain how far the two instruments would be affected by electrical locality alone (52), when both were filled with air of the common density of the atmosphere, which at that time counterbalanced 29·6 inches of mercury: the temperature of the room being 60° F. The two instruments were again placed as in Fig. 8, and the excited glass tube made to approach the cap of A. The balls in this instrument diverged considerably, but those in B were scarcely affected. On bringing the tube into contact with the cap of A, the pith balls struck the side of the receiver, and those in B separated about half an inch, and remained about a quarter of an inch apart after the excited tube had been taken away. The experiment was often repeated and with similar results, excepting that the balls in B did not always remain divergent after the excited body was removed from the cap of A, but generally remained as in the figure.

75. By comparing the results of the above described experiments (71, 72, 73, 74), we discover a material difference between those which are produced when the air in the instrument, A, was attenuated and when at the common density of the atmosphere. When the balls were immersed in the attenuated air (71, 72, 73), their motions were rapid and exceeding irregular, unless great care were observed in bringing the excited body very slowly towards the cap of the instrument, and even when the greatest care was taken, the balls would suddenly strike each other after a moment's separation, and would repeat these motions two, three, and often four times before the excited tube arrived at the cap of the instrument; but when the air within the instrument, A, was of the common atmospheric density, no such vacillancy was exhibited by the balls (74). The divergency was invariably regular and progressive as the excited body approached the cap of the instrument; and in no case did the balls strike each other whilst under the electric influence of the tube or the amber. Another striking contrast in the results of these experiments is observable in the *ultimate maximum* divergency* of the balls in the two instruments. When the air is attenuated in A, the *ultimate maximum* divergency is invariably *greater* in B than in A (72). But when the air in both instruments is of the common atmospheric density, the *ultimate maximum* divergency is uniformly *greater* in A than in B (74.) By taking into consideration every circumstance connected with the above experiments, there was every reason to suppose that when the air was attenuated in the instrument, A, it became a sufficiently good conductor to carry off a portion of the fluid from the cap, straws, and balls of the instrument, and that this fluid was transmitted to the lower instrument, B.

* "*Ultimate maximum* divergency" is intended to express the divergencies in their last stages, or when the excited body is withdrawn from the instrument, A, which is the only period of the experiment in which a just estimate of their relative extent of divergencies can be formed; for although the extent of the divergency of the balls may very easily be ascertained in any stage of the process, the sudden vacillancy of those in A precludes the possibility of knowing to what extent their divergencies are carried.

76. Thinking that if, in place of the salted straws, two better conducting stems were suspended in the instrument, A, the loss of fluid in an attenuated medium would be better observed, I procured for this purpose some fine copper wire, from which proper lengths were taken. One extremity of each piece was bent into the shape of a hook, and the other extremity furnished with a pith ball, and both wires were hung in the axis of the receiver. The instrument was now placed on the plate of the air pump, and before any attenuation was carried on, the excited glass tube was made to approach the cap of the electroscope. The balls separated from each other as gradually as when attached to the straws in the former experiments in air of the common atmospheric density; and when the tube was brought close to the cap of the instrument, the balls struck the sides of the receiver. When the electric force of the tube was not too great, the divergency might be extended to any required degree without the balls striking the receiver; and if the cap were touched by the tube, the balls would remain two inches apart for several minutes after the tube was taken away. To ascertain this latter fact was the principal object of this experiment.

77. The instrument being deprived of its Electricity acquired in the last experiment, the pump was brought into play until the attenuated air in the receiver would just counterbalance one inch of mercury. On approaching the cap of the receiver with the excited glass tube, the wires, with their balls, were strangely disturbed—their motions being more rapid and frequent than those exhibited by the straws—but remained close together when the tube was taken away. By a few trials I got into the method of leaving the balls separated about half an inch from each other; but in no instance could I obtain an *ultimate maximum* divergency to a greater extent: and even this only for a few moments after the excited body was withdrawn, for the balls soon came down to less than a quarter of an inch from each other.

78. I now varied the experiment by first electrizing the wires and balls, and afterwards attenuating the air, as had been done whilst the straws were suspended in the receiver (68, 70). The instrument being placed on the pump-plate, the balls were made to diverge by the application of the excited glass tube. When the tube was removed from the cap of the receiver, the balls were about two inches apart. The pump was now brought into play, and as the attenuation of the air proceeded the balls came closer together; and when the mercury in the gage was reduced to one inch, the balls were less than one quarter of an inch apart. The air was readmitted very gently, but the balls never separated farther than when in the attenuated air.

79. I have repeated every experiment herein described many times over, and have taken every care that I could think of to prevent error in the results. The experiments described (in 68, 70, 78) are those alone which can be considered as repetitions of that on which so much theorizing speculation has been ventured; and as the experiment requires no very refined experimental dexterity, it is not difficult to discover that there is some unaccountable error in Lord Stanhope's description of the results

(60 *h*); for in no instance have I yet seen the least tendency to divergency of the balls by readmitting the air into the receiver; nor indeed can I see any cause for the appearance of such a phenomenon, unless it were from the greater degree of buoyancy which the balls would experience in the dense than in the rarified air.

80. The experiments detailed (in 71, 72, 73, 76, 77) may be regarded as perfectly original, and such as the nature of the inquiry obviously required. In every one of these there has appeared almost indubitable proof of a *loss* of fluid through the medium of the attenuated air—a conclusion which will be strongly corroborated by the next described experiments.

81. The electroscope, with its wire indices and balls, was placed on the pump-plate, and the balls made to diverge, sometimes by the application of the excited glass tube, and at others, by the excited amber; the air in the receiver not being molested by the pump. The standard divergency was two inches, when the exciting body had been taken away. The object of these experiments was to ascertain whether the balls, when electrized to the same extent, would retain their divergency for a longer period when the air was undisturbed, or when it was attenuated by the pump. The temperature of the air of the room in which they were made was 60° F., and the barometer stood at 29·6 inches. The results of the experiments are shown by the following table. The left-hand column shows the standard distance of the balls at the commencement of each observation, and the character of the excited body employed. The second column shows the time required for the loss of the standard quantity of electric action, when the balls remained in an atmosphere counterbalancing 29·6 inches of mercury. And the third or right-hand column shows the time required for the same loss of electric action, when the air about the balls was reduced to a pressure equal to that of one inch of mercury.

Table of Experiments, exhibiting the Time in which an Electrometer lost a standard quantity of Electric Action, in Aerial Media of different densities.

The standard repulsive distance of the pith balls, 2 inches, when excited by	Time required for the total loss of the standard quantity of electric action.	
	In air balancing 29·6 inches of mercury.	In air balancing 1 inch of mercury.
Glass, . .	5 minutes.	$1\frac{1}{2}$ minutes.
Amber, . .	4 minutes.	$1\frac{1}{4}$ minutes.

82. Having now satisfied myself that the *lessening* of the divergency of the balls, when the air in the receiver became attenuated (60 *d*, 68, 70, 71, 72, 73, 77, 78, 81), was owing to a real loss of the electric fluid which they sustained; and that the total disappearance of the standard quantity was much facilitated when the air in which the balls were immersed was attenuated (81). I now thought it possible that the total disappearance of the electric action on the balls might be effected in a still less period of time than that shown in the table (81), by *diluting* the electric fluid with which the instrument was charged with fresh portions of air. For this purpose the receiver was made quite dry and warm; and when placed on the pump-plate, an electric charge was given to the balls from the excited glass tube, which caused an *ultimate maximum* divergency of two inches and a quarter. This being accomplished, the apparatus was permitted to remain unmolested till the divergency entirely disappeared, which occupied *six minutes and a quarter.* The instrument was now charged again to the same standard of divergency as before. As soon as the glass tube could be got out of the hand, the pump was brought into play, and the mercurial gage brought down to three inches. The air was readmitted, and again pumped out until the balls came close together. The air was now again readmitted very slowly, but the balls did not diverge again, so that by this one *dilution* of the electric fluid, the whole of its action on the balls entirely disappeared. The time occupied was *forty-seven seconds.*

83. It had occurred to me, at various times during these experiments, that there was a probability of even *seeing* the electric fluid make its escape from the balls, through the attenuated air in the receiver, provided the room was darkened; but, being at that time otherwise engaged in the evenings, a considerable period elapsed before I had an oportunity of ascertaining the correctness or incorrectness of this idea. Eventually, however, the experiment was made, and with the anticipated success. The electroscope with the wire indices (76), was placed on the pump-plate, and the air within attenuated till it would just counterbalance one inch of mercury. The glass tube was then excited, and brought towards the cap of the instrument, and the wires and balls were agitated in the manner already described (77). The room was now darkened and the tube again excited, and then brought to the cap of the electroscope. Sparks immediately appeared from both balls, darting in a very beautiful manner to the sides of the receiver, at nearly the same height as the balls were suspended; and from these places exhibited luminous streaks down the sides of the glass to the plate of the pump. This beautiful and conclusive experiment I was induced to repeat many times, during which I frequently observed three, and sometimes four, of these streaks of electrical light by one application of the excited tube. The streaks of light exhibited in this experiment are tolerably represented by the crooked lines, *o b*, *o b*, *o c*, *o c*, in Fig. 9.

84. When excited amber was employed instead of the glass tube, the light was seen in the receiver as decidedly as in the last experiment (83), but its appearance was very different, being much fainter and not in such well defined lines. Both of the pith balls appeared beautifully illuminated, especially on their lower sides. I tried to vary the light by placing pointed wires on the pump-plate, as represented by *w w*, Fig. 10. By this means the light above the balls became a little brighter than before, and extended farther from them, always inclining towards the points, *w w*; but in no instance was there much brilliancy, nor any definite streaks of light, as when the glass tube was used. The figure of each portion of light had some resemblance to an inverted cone, as represented at *b c*, Fig. 10, and did not appear very unlike the tail which some comets have exhibited.

85. It may here be proper to state, that I have invariably obtained more accurate results from the metallic wire indices or stems (76) of the electroscope than from the straw soaked in a solution of common salt (60 *a*). With the former the loss of fluid is regular and uniformly the same in every experiment made under similar circumstances, but with the latter the results are much influenced by the hygrometric condition of the salt, and also by the asperous surfaces which it gives to the straws. Dry straws, which are not salted, give different results to those which are so treated; and gold leaves give still different results to either straws or wires. These and many other circumstances connected with electrometric experiments in air of different degrees of density, and some novel facts, will be more particularly noticed in my second Memoir, which will shortly be submitted to the consideration of the Electrical Society. As far as I have hitherto proceeded, there have not appeared any facts which militate against the operations of a repulsive power in the display of electrical phenomena; but, on the contrary, there is much evidence in favour of the existence of that attribute. It will have been observed that Lord Stanhope's experiments are totally inconclusive on this point; and it is somewhat remarkable that his Lordship, who has made so many excellent experiments on the polarization of bodies by electrical locality, should in this instance have confided so much on one single experiment without even the slightest variation; for his Lordship says, in the appendix to his work, at page 235, that those "experiments were performed with *positive* Electricity only." And again, at page 236, "it was quite unnecessary to make any similar experiment with *negative* Electricity." From these statements, and from the great confidence which Lord Stanhope placed on this single experiment, it is obvious that his Lordship had not the slightest suspicious of the escape of fluid in the attenuated air; and from the implicit sanction which this experiment has generally met with amongst philosophers, it would seem that not the remotest idea of the fact has hitherto been entertained.

86. It will have been observed that in none of the experiments hitherto described have I attenuated the air in the receiver to a greater extent than as a counterbalance

to one inch of mercury; whereas, in Lord Stanhope's experiment, the density of the air was reduced so as to counterbalance only one-quarter of an inch of mercury (60 *d*). I have adhered to the former standard density of the air in the receiver on two different accounts. First, because when the mercury in the *short* barometer gage had fallen to one inch, the cork balls had approached to about a quarter of an inch of each other (68), which is the shortest distance between the cork balls in his Lordship's experiments (60 *d*). Secondly, as the generality of pumps which are in the hands of experimenters will reduce the mercurial column to one inch, and but only a few of them sufficiently accurate to bring it down to a quarter of an inch, I have thought it better to record the experiments under those circumstances in which they may be repeated with the greatest facility, and within the range of those means which are at command by the greatest number of experimenters. It will be necessary to mention, however, that I have attenuated the air about the electrized balls to a much greater extent, but, as might have been expected, the loss of the fluid was greatest as the mercurial column in the gage shortened; and I have often found that, when the air was counterbalanced by less than half an inch of mercury, the balls would come down to one-tenth of an inch from each other. Much exactitude, however, will always be required in experiments of this kind, for as the loss of the electric fluid will depend both on the *attenuation* of the air and on the *time* occupied in pumping (81, 82), it is obvious that the divergency will be lessened on both these accounts; and the distance between the balls, when the mercurial column is reduced to any *standard altitude*, will depend upon the *time* occupied in bringing the air to the given degree of attenuation.

87. The time required to bring the mercurial column of the gage down to a quarter of an inch is very great, when compared with that necessary to reduce it only to one inch, even when the best pumps are used. In the experiments I have described, the time of pumping was particularly attended to—the standard being *thirty seconds*—which, under those circumstances necessary to guard against the agitation of the apparatus, was the shortest period that the pump which I employed would allow to bring down the mercury to one inch. Under all these circumstances it is obvious that much caution is necessary whilst carrying on experiments of this delicate nature, and that the standard attenuation of air which I have employed is much better calculated to give exact results than when the attenuation is carried on much farther; and as the propagation of novel facts is always facilitated by simplifying the means of producing them, I have been anxious to place these within the reach of every experimenter, hoping they will be the means of removing some of those theoretical prejudices which have so long rested on the *report* of one solitary experiment.

W. S.

Westmoreland Cottage, Nov. 26th, 1837.

ON THE IDENTITY OR NON-IDENTITY OF ELECTRICITY AND MAGNETISM.

TWENTY-THIRD MEMOIR.

(Read March 3rd and December 19th, 1838.)

Different opinions of Philosophers on this Topic—Experimental Examination of those Phenomena which are supposed to favour the Hypothesis—Examination of M. Ampere's Hypothesis—The Polar Forces of Hard Steel Magnets unvanquishable by Electric Currents—The inefficiency of Electric Currents in Magnetizing Hard Steel to a high degree of Power—The distribution of Magnetic Force exhibited by Steel Magnets and by Loadstone not imitable by Electric Currents.

1. In the first Memoir which I had the honour to present to this Society, I endeavoured to elucidate those fundamental principles of Electricity which appear obviously developed by an extensive series of illustrative phenomena, and well calculated to afford an easy explanation of the nature and peculiarity of electric action. There still, however, remains one very important theoretical point on which I have not yet touched—a point which is yet wavering under the dominion of vacillating opinion without any party venturing a demonstration of his peculiar ideas, or indeed showing much, if any, reason for entertaining them.

2. The discovery of the identity of lightning and ordinary electric discharges by Franklin, and the well established facts of lightning depolarizing compass needles, reversing the polarity of others, and producing other remarkable phenomena, were events that have long ago led philosophers to imagine that Electricity and Magnetism are not distinct powers of nature, but that more probably they emanate, in different forms, from one and the same physical cause. The apparent similarity of the attractions and repulsions in Magnetism and Electricity has also been considered as favourable to the hypothesis.

3. It is now more than half a century ago since the celebrated Father Beccaria ventured an opinion that the electrical and magnetic powers are identical. " Are not these peculiar effects of the electric fire with respect to Magnetism," said this eminent philosopher, " so many proofs which corroborate my former conjectures, that the peculiar magnetic force observed in *loadstone* is to be attributed to either atmospherical or

subterraneous strokes of lightning, and that the *universal systematic* properties of magnetic bodies are produced by a universal systematic circulation of the electric element ?"* This hypothesis of the illustrious Italian was not much attended to till the discovery of Electro-Magnetism, which happened nearly fifty years afterwards, when it was again broached, as a new idea, by M. Ampere. Since that time the hypothesis has gained many proselytes, though there be still some philosophers who do not entertain that opinion; and as Electricity has latterly produced many phenomena, whose true cause can only be understood by a proper solution of the problem which this disputed point has created, a strict investigation of the various circumstances connected with it can hardly fail to be interesting to the Electrical Society, I have therefore devoted the whole of this Memoir to that particularly important subject, in which it will be found I have collected, examined, and arranged the most striking instances of analogy in Electricity and Magnetism, and have also pointed out many phenomena in which they as obviously disagree. I have contemplated the whole as profoundly as I have been able, and have discussed the various topics as I have proceeded with freedom and candour in the manner following:—

4. If one of the poles of each of two magnets be presented to each other, a tendency either to recede from, or approach, each other is immediately manifested, accordingly as these poles are similar or dissimilar respectively; and because similar and dissimilar electrized bodies evince corresponding tendencies to move *from* or *towards* each other, the two sets of phenomena have been regarded as marking a strong analogy, and have been held forth as evidence in favour of the identity of the magnetic and electric agents. But before these or any other supposed analogies be permitted to enter into any code of physical laws, they ought to be examined with the most rigid scrutiny and exactness. The phenomena ought not only to be compared with each other, but each individual event should be traced, as closely as circumstances will permit, to the nearest cause of its production, and in what manner it would be affected by varying the conditions of the experiment; and, in the question before us, it is only from such close investigations as these that data are to be obtained which can be esteemed of much intrinsic value.

5. In contemplating the phenomena I have been speaking of in the manner proposed, let it be supposed that $n\ s$, $s'\ n'$, Fig. 1, Plate XVI. are two magnetic needles, each suspended by a fine thread; and that p and n, Fig. 2, are two dissimilar electrized balls, suspended in like manner. Then, because of the magnetic poles, $n\ s'$, $n's$, which are opposite to each other being of different kinds, they will approach each other until they come into contact; and a parallel phenomenon will be exhibited by the dissimilarly electrized balls, $p\ n$. Thus far the analogy appears to hold good. Our conclusions, however, are not to be drawn from these facts alone, for the motions

* Treatise on Artificial Electricity, by Father Giambatista Beccaria, page 310. English edition, London, 1776.

already performed are the mere preliminaries to the display of other phenomena which demand still greater attention, and reveal the operation of other attributes than those which brought the bodies together. The electric balls, *p n*, very shortly after the first contact, separate from each other; and if their first electric conditions were of equal degrees *above* and *below* the common standard, or neutral state, they would *neutralize* each other's action, and their fibres of suspension would hang parallel to each other; but if their first electric conditions were not of equal degrees above and below the natural standard, both balls would remain either *positively* or *negatively* electrical accordingly as *p* or *n* exhibited the greater degree of electric tension prior to the first contact. In either case the balls would display a tendency to recede from each other, and diverge their fibres of support.

6. Now the motions last exhibited by the electric balls find no parallel phenomena in the magnetic poles, *n s' n s'*, Fig. 1, which still cling together without evincing the least tendency to separate; instead of which, it is a well known fact, that the longer those poles are permitted to remain unmolested the greater degree of force would be required to separate them. Hence then, without entering into any theoretical disquisition, these electric and magnetic phenomena are so obviously dissimilar, that instead of being susceptible of inferences in favour of an identity in the operating causes, they have an obvious tendency to bias the mind to the very opposite conclusion.

7. Let the two electric balls, *p n*, Fig. 3, be suspended on the opposite side of a fixed ball, B, which is in the natural electric condition. The electric bodies, *p* and *n*, will immediately approach B, and, after contact with that body, they will recede from it. When the body, B, is insulated, and the bodies *p* and *n* differ in degree of electric tension *above* and *below* the natural standard respectively, all the three bodies remain electrized after contact; and *p* and *n* exhibit a tendency to recede from B. If, on the other hand, *p* and *n* are of equal degrees of electric tension *above* and *below* the natural standard, they will neutralize each other through the medium of B, and B also will remain neutral. If the body, B, were uninsulated, it would be a matter of no consequence in what manner *p* and *n* were electrized, they would both become neutralized by contact with that body. Here then we have three conditions under which the electric balls, *p* and *n*, would approach B by electric action, but in no case would they be retained in contact with that body. In every variation of these experiments, the bodies, *p* and *n*, would have their electric energies considerably deteriorated by contact with B, and in some cases those energies would totally vanish by such contact, however powerfully they might previously have been displayed.

8. Let now a parallel experiment be made in magnetics by suspending two light bar-magnets by threads, as represented by Fig. 4. When the inferior dissimilar poles, *n s'*, hang on the opposite sides of a soft iron ball, *i*, as in the figure, they immediately approach that ball; and when they have once come in contact with it they remain

attached to it; and the longer they are left undisturbed the greater is their tendency to remain there, so that the contact, instead of diminishing the attractive force, absolutely increases it. How very different are these events to those which occur by electric action. In every case of contact by magnetic attraction, the forces which bring the bodies together become exalted in some proportion to the closeness of contact; and in no case are those forces impaired by *time.* The electric attractive forces, on the contrary, are invariably and immediately impaired by the bodies touching one another. In some cases they are suddenly and totally neutralized; and in no instance are they of long duration, independently of a continuous exciting process.

9. Electro-polarization (52, page 394) has an apparent analogy in Magnetism, but the different ways in which the experiments may be varied lead to results which show an obvious difference in the causes producing them. The nearest responsive fact is the polarization of soft iron by placing it in the vicinity of a permanent magnetic pole. If, for instance, the piece of soft iron, *s' n'*, Fig. 5, be placed near to the magnetic pole, *s*, of the steel bar, *s n*, a magnetic polarity will immediately be displayed in the iron bar, and arranged as indicated by the letters—viz. the south pole, *s*, of the magnet, *n s*, will cause a north pole in the vicinal extremity, *n'*, and a south pole in the remote extremity, *s'*, of the iron bar; but if the north pole of the magnet be presented to the soft iron, as represented by Fig. 6, the order of polarity in the iron will be the reverse of that in the former instance, though still in accordance with the same law; for in both cases the poles, in the permanent magnet, occasion poles of the opposite kind to be exhibited in the nearest extremity of the iron, and polarity of the *same* kind in the remote extremities of the iron.

10. The circumstances under which the magnetic polarity thus displayed by pieces of soft iron bears so strong a resemblance to those necessary to the production of electro-polarity (Figs. 3 and 4, Plate XV.), that a superficial observer might easily be led to imagine that the same agency was in operation in both cases; but here, as in the cases already described (5, 6, 7, 8), a close investigation of these phenomena, and a correct view of those which a variation of the circumstances productive of them exhibit, lead to very different inferences. Let us, for instance, permit the pieces of soft iron, as in Figs. 5 and 6, Plate XVI. to touch the permanent magnetic poles to which they are presented. The steel and iron would remain as decidedly polar as before; and the remote poles, *s'* and *n'*, of the two pieces of iron, and *n* and *s* of the steel bars, would display still stronger polar forces than prior to the contact. These facts have no parallel in Electricity; for if the electric bodies, P and N, Figs. 3 and 4, Plate XV. be brought into contact with the bodies, *n p* and *p n*, to which they are respectively presented, the phenomena of polarity ceases to be exhibited; each pair of bodies immediately becomes similarly electric throughout—the one pair, Fig. 3, being all in

an electro-positive condition, and the pair, Fig. 4, being in an electro-negative condition on every part of their surfaces.

11. The electric phenomena displayed by bringing the bodies, P and *n p*, Fig. 3, and N and *p n*, Fig. 4, Plate XV. are easily explained by supposing an introgression of fluid *from* the relatively positive to the relatively negative bodies of each pair; but it would be exceedingly difficult to understand how the magnetic bodies maintained their polarity by any *similar* distribution of a fluid, or of any other physical agent; for whatever may be the nature of the magnetic agent, it is obviously more determinedly fixed or accumulated in the extremities of ferruginous bars by close contact than when those bodies are at an appreciable distance from one another. Hence we discover that the magnetic and electric forces, which, at certain distances, effect such a similarity of phenomena in bodies situated in their respective localities, are productive of no corresponding facts when the approximation of those bodies is sufficiently close. Neither do the phenomena agree, which the newly-magnetized and electrized bodies exhibit, after they have quitted those original magnetic and electric bodies whereon the respective disturbing forces reside; for, after the separation of *n p* and *p n*, Figs. 3 and 4, Plate XV. from P and N respectively, the former would exhibit *positive* and the latter *negative* electric action; but the pieces of iron, Figs. 5 and 6, Plate XVI. would lose all traces of magnetic action when once they were sufficiently removed from the localities of the magnets to which they had been attached.

12. If it can be imagined that, by substituting steel for the pieces of soft iron, in Figs. 5 and 6, Plate XVI. an analogy to the phenomena exhibited by electrized bodies would have been more apparent, by the steel retaining magnetic action after quitting the disturbing magnetic poles, I would observe that its retaining some trace of magnetic action is a fact which cannot be denied; but in that case the steel would remain polar, as is always the case with magnetic bodies; and, as no trace of polarity would be exhibited by the electric bodies, but, on the contrary, an uniformity of electric action would be discoverable over every part of their respective surfaces, the *supposed* analogy again loses its support, and as decidedly fails in this instance as in those previously discussed. Moreover, the pieces of steel would retain their polarity unimpaired, even after long continued contact with other bodies; whereas the electric bodies would lose all trace of electric action by the slightest touch with uninsulated conductors.

13. A globe of steel may be made to exhibit *permanent* magnetic polarity when far removed from every disturbing force; but the same globe will not maintain any corresponding electric action. A plate of glass will exhibit electro-polarity on its opposite surfaces for some considerable time after it has been removed from the exciting apparatus; but magnetic polariry is not known to be exhibited by glass. If then the magnetic and electric elements be identical, why this capricious selection of bodies for the display of these parallel phenomena? The electric forces will attract all kinds of

matter without exception; but the magnetic forces appear to be exceedingly select in this particular, operating on certain kinds only. Coated glass, whatever may be its form, affords no *permanent* electric attractions which are in the least comparable with the attractions exhibited by magnetic bodies; for if a metallic arc connect the two sides of a Leyden jar, the electric forces immediately disappear; but an iron arc, connecting the poles of a horse-shoe magnet, is permanently held there unless removed by mechanical violence; and the longer it remains undisturbed by extrinsic force, the more vigorously is it attracted by the poles; and there is no known substance whatever, by which the poles of a magnet may be connected, that will in the least deteriorate their powers.

14. Those few kinds of elementary matter on which magnetic attractions are known to be exerted, display no distinction of respect for the *north* or *south* polar forces, being attracted indiscriminately, and to the same extent, by both. Very different, indeed, are the nice discriminations of the *positive* and *negative* electric forces, manifested in an almost endless variety of phenomena, every one of which teems with interest in the contemplations of the philosopher, and beautifully characterizes the agency of their production. If, for instance, an intimate mixture of sulphur and red-lead be indiscriminately projected through the air to a series of *positively* and *negatively* electrized surfaces, the powders will be separated from each other by the dissimilar electric forces into whose spheres of action they are thrown; and the sulphur and red-lead will respectively be found at the possitive and negative surfaces, exhibiting a peculiarity of arrangement not known to be accomplished by any other kind of physical agency.* Similar selections are uniformly exhibited by electric forces whenever the particles of compounds on which they operate are sufficiently voluble to be put into motion by them, or are held together by inferior powers. Every individual electro-chemical decomposition appears to be an instance of this kind of action, and demonstrates the peculiarity of this important fact.

15. It has been said by M. Œrsted, that the only difference in the electric and magnetic forces rests in the different degrees of tension or activity, the electric being the more active or vigorous in its operations; and this hypothesis has been attempted to be supported by M. Ampere and other philosophers, whose opinions on this subject will long command respect. But I must confess that I can discern no satisfactory discrimination of this kind, nor am I acquainted with any facts that are even in the least favourable to it. It is well known that electric attractions are the most powerful when the bodies exhibiting them manifest the greatest degree of tension in the display of all other electric phenomena. The spark, for instance, is shown to the

* This fact was first shown by Leightenberg. Cavallo and Bennett, especially the latter philosopher, have extended the original experiments of Leightenberg, and varied them in a variety of pleasing and interesting ways.—*Bennett's Experiments on Electricity. Derby,* 1789.

best advantage when the electric body, whence it proceed, exhibits the greatest degree of attraction; and the charge of a jar is accomplished in the shortest period of time, and with the greatest degree of facility, under similar circumstances. Moreover, when electric discharges are performed, either from a single jar or from a battery of jars, the striking distance is greatest, the flash is the most brilliant, the noise is the loudest, the physiological effects are the most powerful, and, in fact, every phenomenon is exhibited under the most advantageous circumstances and in the most perfect manner, when the jar or battery is in the most suitable condition for a display of its attractive energies.

16. But now let us inquire into the *extent* to which electric attractions are usually exhibited. Has any Electrician ever seen a prime conductor (which always shows attraction more powerfully than any other electric apparatus)* support, by its electric energies alone, a single *ounce* of any kind of matter? I presume not. If then with this insignificant attracting force, Electricity be prepared for a display of some of its most splendid and terrific phenomena—the production of vivid light, intense heat, the noise of thunder, and the destruction of animal life—and that Magnetism proceeds from the same cause or agency, it seems natural to ask, why it is that similar phenomena are not exhibited to the same, or even a greater extent, by a magnetised body, whose attractions are ten thousand times ten thousand greater than any ever witnessed in Electricity? These important questions, which stand so prominently and essentially in the path of investigation, demand the most profound contemplation of the philosopher, and must not be passed over in silence by those who are endeavouring to identify the electric and magnetic powers. We have yet to learn the mode of producing a *magnetic spark*, and are totally ignorant of the sensation communicated by a *magnetic shock*. And *Magnetic Chemistry* is so profoundly obscured from our knowledge, that no one knows even of its existence.

17. If our reasoning be permitted to rest on facts alone, independently of favourite notions and ingenious hypothesis, which are too apt to captivate the imagination of the superficial observer, and sometimes even to sap the understanding of the more studious in science, the obvious contrasts in the phenomena, presented by Electricity and Magnetism, enforce themselves upon our notice too powerfully to be misunderstood. Even the attractions themselves, in which *alone* the appearance of analogy exists, are so exceedingly dissimilar, so truly distinct from one another, that their peculiar characteristics are well defined and easily discernible, and cannot be mistaken by those who devote to them a proper and sufficient degree of attention.

18. An insulated electrized globular body *radiates* its attracting influence on every side alike, when surrounded by an uniform medium, such as the atmospheric air, as

* Two electrized panes of glass would be an exception, but even in this case the attraction is perfectly insignificant when compared to magnetic attractions.

may be understood by Fig. 7, Plate XVI. which may represent a great circle of the globe with its radiating electric force; but a magnetized globe, similarly situated in space, exhibits no such radial influence, for being polar on opposite points (*n s*, Fig. 8) of its surface, the greatest *disposable** attracting forces are exerted about those polar regions, and especially in the line of their axis continued. At right angles to that axis, in the plane of the equator, *e e*, the polar forces, by their mutual attractions, nearly balance one another, neither of them exhibiting much *disposable* influence on exterior bodies. Another great characteristic distinction in the display of the electric and magnetic forces by these bodies appears to be this—the electric force is wholly *disposable*, and ready to be exerted upon, and even *transferred* to, other vicinal bodies; whereas the magnetic forces are neither *transferable* nor wholly *disposable*, for no magnet has yet been known to have its power impaired by contact with unmagnetized bodies, and in no case is the whole of its attracting power exerted upon a vicinal body.

19. I have been exceedingly anxious to discover, if possible, some facts which might afford analogies whereon to fix a basis of reasoning on the identity of these physical agents; but although I have met with some further phenomena far from being uninteresting in the discussion, a close examination of their true character has shown their evidence in favour of the supposed identity to be of no more value than that afforded by the facts already noticed.

20. If there be one electric apparatus more than another whose action resembles the action of the magnet, it is the dry *electric column*, whose polar forces are more

* It appears by the distribution of iron-filings, when strewed on paper above a bar magnet, that a considerable portion of the *north* and *south* forces are engaged in attracting one another, as shown by the curve lines assumed by the filings; and consequently are not employed, or at least very sparingly so, in any attractions which the magnet exercises on foreign bodies, such as pieces of soft iron, magnetic needles, &c., placed a few inches distant from its extremities, and in a line with its axis, or indeed opposite to any other part of its surface; and, although much more of the magnetic force is brought into play as the iron is brought nearer, and most of all when it is in contact with the pole of the magnet, there is still a considerable portion of force which cannot be exerted on this foreign body, because of its being engaged with the opposite force about the surface of the steel, which lies between its extremities, and especially that which is situated near to its centre. For convenience then, I call that portion of the magnetic force which lies about the equatorial part the *engaged force*, and that which is brought into play on foreign bodies the *disposable force*.

The *disposable force* of any magnet may be diverted from its original directions of action by the approximation of ferruginous bodies; and, in some instances, nearly the whole of it may be drawn from a body on which it operates without moving either the magnet or the body. To illustrate this point, let a bar magnet be placed six or eight inches distant from the pivot of the needle, and at right angles to its direction. The *disposable* force of the magnet will deflect the needle to some considerable number of degrees. Now place on each side of the magnet, parallel to it, and about three inches distant from it, a piece of soft iron, about its own shape and size: the deflection of the needle will lessen considerably, showing that a portion of the *disposable* force has been diverted from its action on the needle. Now, bring the pieces of iron nearer to the magnet, and the deflection again decreases; and when the pieces of iron are brought into close contact with the magnet, one on each side, from end to end, nearly the whole of the *disposable* force will be exerted on the iron, and but very little of it, if any, will reach the needle so as to cause a perceptible deflection. Now, in this case, the extremities of the magnet are still untouched by the iron, and are consequently as much exposed to the needle as when the iron was not present; notwithstanding which it is obvious from the experiment, that the *disposable* force which before deflected the needle has now taken another direction, and is employed in polarizing the pieces of soft iron. The disposable force of the magnet, however, although it cannot now reach the needle with a sufficient degree of formidableness to accomplish deflection, is not entirely engaged by the iron, a residuum still remaining, which is detected by bringing the needle nearer to the magnet.

uniformly and permanently exhibited than those of any other electrical instrument. But the attractive and repulsive powers of this instrument, like those in all other electrical arrangements, are exceedingly feeble when compared with the gigantic powers of a magnet; they are, moreover, directed towards, and operated upon, every kind of matter without distinction; whereas, the magnetic attractions and repulsions, notwithstanding their vigorous action on ferruginous bodies, are, with the exception of one or two of the metals, perfectly inert on all other kinds of matter. The attractions and repulsions of the electric column are productive of vibratory motions in pendulous bodies properly situated between the poles, which show that the vibrating body changes its electric condition at every contact with either pole of the instrument, and accommodates itself to the attractive influence of the opposite pole. When the pendulous body has come into contact with the positive pole, it acquires an electro-positive condition, and is repelled to the negative pole, where it deposits its charge and becomes electro-negative. It is now again under the attractive influence of the positive pole, to which it is compelled to make another journey, and *from* which it receives a new charge and an immediate succeeding repellent impulse, which again directs it to the negative pole; and in this manner the suspended body performs its vibratory motions, being in an electro-positive condition whilst travelling in one direction, and in an electro-negative condition whilst travelling in the other. By these means a *pulsatory current* permeates the pile from the negative to the positive pole, the fluid being transported through the air, from the latter to the former, by means of the pendulous body.*

21. Besides the pendulous motions alluded to, the dry electric column is productive of physiological and chemical phenomena, will emit sparks and charge coated glass and other inferior conductors as decidedly as charges are produced by the machine; all of which are so perfectly distinct from, so decidedly foreign to, any known capabilities of the magnet, that there is not to be found one solitary trace of analogy in the performance of the two kinds of apparatus. The attractions and repulsions are the only phenomena in which there is a *shadow* of resemblance, whilst in *reality* even this faint analogy has obviously no special existence. The delicate electric forces which alternate the condition of, and give vibratory motions to, the pendulous body, find no similarity of action in the majestic attractive forces of the magnet which select those of their own species only, whose coeval polar affinities mutually exalt the action and constrain the attracted body to assume a determinate polar condition, and prevent its escape from the vigorous influence of the pole to which it is first attached. Hence, as no vacillancy in the magnetic condition of the attracted body is produced, the grand essential to vibratory motion has no existence in magnetics; nor can any such loco-

* As this discussion requires experimental facts rather than theoretical opinions, I have not, in this place, entered on the doctrine of the dry electric column. It is possible I may have occasion at some other time to enter fully into the philosophy of this interesting apparatus. (See eighth Memoir.)

motions, as those exhibited by the electric column, be produced by any known self-acting powers of the magnet.

22. If we are to look for the supposed identity of Electricity and Magnetism amongst electro-magnetic phenomena, we are still as far from arriving at satisfactory conclusions as in any other branch of science. It is true we here find some of the most striking and interesting affinities which Electricity and Magnetism have hitherto developed—affinities which will ever link these sciences together in the firmest bonds of physical union, though by no means identifying the elements by which the phenomena are produced. Each elemental agent plays its own part in the production of electro-magnetic phenomena as decidedly as in those of Magnetic Electricity, whose display is accomplished by the reciprocal excitement.

23. From the attractions and repulsions exhibited by wires carrying electric currents, M. Ampere was led to imagine that all magnets owe their influence to an unremitting circulation of the electric fluid; an hypothesis so exceedingly ingenious, and so eminently calculated to favour the expectations of some philosophers, that there can be no astonishment excited by its gaining proselytes amongst those whose minds were already predisposed for its reception. But notwithstanding the respect which is due to the talents of those philosophers who have favoured Ampere's views on this topic, I must candidly confess that the hypothesis has always appeared to me to be much easier to acknowledge than to understand. In the present investigation I have considered experimental facts as the only data on which I can proceed with any chance of success of arriving at a close approximation to true theoretical inferences. I have, therefore, neither ventured an opinion of my own, nor permitted the views of others to influence the inquiry.

24. The imaginary electric currents to which Ampere refers all magnetic action, lead us to inquire into the character and situation of their source, and by what means they can be supposed to be *perpetually* and equably maintained, either on the surface or within the body of a steel bar. Here it is that we are led to enumerate and examine all the known artificial sources of electric excitement, and endeavour to trace their influence to the operations of permanent steel magnets. Independently of *magnetic* excitation, we know of only three sources of electric currents—viz. frictional, Voltaic, and thermal; for, besides these four, there are no other sources known.* Hence if a bar of steel which exhibits *permanent* Magnetism has that power conferred upon it by the influence of electric currents, which must necessarily be as durable as the magnetic action itself, to which of these sources are we to look for the *supposed* actuating currents? Or are there other sources of electric currents of which we are yet entirely ignorant? But from whatever source those imaginary currents may be supposed to proceed, that source must necessarily be situated either on the surface or within the body of the steel. The idea of electric currents being excited by *fric-*

* The dry electric column is here omitted.

tion amongst the particles of the solid metal, is too absurd to be entertained for a moment; and the conditions necessarily required for the production of *Voltaic* currents are no where to be found in the steel; hence our inquiries are necessarily limited to *thermal* excitation alone.

25. That thermo-electric currents are producible in every piece of metal, whether pure or compound, is a fact which I have proved by very extensive experiments some years ago.* But it must be understood that to produce an electric current by any means whatever requires a co-existent motion in some of the elements employed during the whole time the current is flowing, unless it be of a momentary duration only, and the effect of an impulse, in which case the current may continue to flow for a short time subsequently to the terminal exciting impulse. When a current is produced by an electric machine, the glass cylinder, or plate, as the case may be, is necessarily kept in motion. When a Voltaic combination is the electric source, the *liberated* elements of the liquid in the battery are put into motion, and become vehicles for the transportation of the electric fluid to and from the solid parts of the arrangement; and a thermo-electric current depends upon the motion of the calorific matter, for when that element is perfectly at rest in the combination the electric current ceases to flow.

26. From the above considerations it appears that a perpetual propagation of thermo-electric currents on the surface, or within the body, of a steel magnet, would require a perpetual motion of caloric within its mass, which motion, unless the production of some hidden, mysterious, and unsuspected agent within the steel, would require as continual an influx and efflux of the calorific element from and to the surrounding medium. Moreover, the laws of Electro-Magnetism require that the direction of the electric currents should be at right angles to the axis of the steel bar; and the ingenious author of the hypothesis has ventured to assert that their route is in that direction, in a series of parallel spirals round its surface.† Such then are the necessary conditions upon which Ampere's hypothesis essentially depends; and being now, probably for the first time, disrobed of their mysterious habiliments, I must necessarily resign the glory of their *discovery* to those philosophers who still entertain the idea of their existence in the steel, and who may possibly be enabled to penetrate the subject still deeper than I have investigated it. But before I quit this important topic, I will mention a few more facts which to me have appeared of some consequence, and can hardly fail to be interesting to others who may be induced to pursue the inquiry.

27. If the temperature of one extremity of a steel bar be elevated, and by that process electric currents become excited, those currents would necessarily be more powerful than any which can be supposed to exist in the metal at its natural temperature; and if the other extremity of the steel were to be heated, and again thermo-electric currents be produced in it, these latter currents would be propagated in the

* Philosophical Magazine and Annals of Philosophy, vol. x. page 1. (See second Memoir.)

† Annales de Chimie et de Physique, tome xv., and Ampere's Recueil des Observations Electro-dynamiques.

opposite direction to the former, and consequently the magnetic forces which they brought into play would be exerted in the reverse order to those which the first currents excited; and these artificially excited electro-magnetic forces being more powerful than any which the *supposed* natural electric current could produce, they would predominate over these latter, and give new energies to the bar, reversing its poles in accordance with the directions of the currents. But on making the experiments, and carefully examining the phenomena, I find that no such corresponding changes have taken place in the polar forces of the magnet; and, although the poles themselves are considerably molested during the unequal temperature of the extremeties and other parts of the magnet, and are removed from their original positions by the heating process, they do not assume those positions and variation of force which the thermo-electric current would necessarily give to them were they governed by no other influence.* Hence I infer, that thermo-electric currents do not constitute the sustaining power of the magnet.

28. I next subjected a steel bar magnet to the influence of electric currents proceeding from a Voltaic pair of copper and zinc. The Voltaic combination was of the cylindrical shape and size, which, as is well known, I have long employed for electro-magnetic purposes; the zinc being surrounded by brown paper or calico to prevent contact with the inside of the copper, and the whole placed in a pint porcelain jar—the exciting liquid being a solution of nitrous acid in water. The magnet which I em ployed was of hard cast steel, cylindrical, and about six inches long, and three-fourths of an inch in diameter. It was well polished on an emery wheel, and of considerable power. It would lift, by one of its poles, a piece of soft iron of its own weight. A piece of soft iron of precisely the same figure and dimensions as the magnet, was also provided. A single helix of copper wire, No. 13, of the same length as the magnet, was formed on a hollow pasteboard cylinder of sufficient width for the easy introduction of the magnet or iron. With these preparations, and a compass needle furnished with an agate cap, and supported by a fine steel point, the experiments were carried on in the following manner:—

29. When the meridian line of the compass-box had been adjusted parallel to the needle at rest, the helix was placed on the eastern side of its pivot, with its axis in the same horizontal plane as, and at right angles to, the axis of the needle—the nearest extremity being twelve inches from the needle's pivot. Fig. 9 is a representation of the arrangement, where C is the compass-box, H the helix, and B the battery. Before the battery connections were made with the helix, the magnet was introduced to the interior of the latter with its marked end nearest to the needle, consequently

* At the time this Memoir was first drawn up, only a few experiments had been made on this part of the inquiry, the general results being such as are described in the text. But, whilst writing a fair copy for the press, I was led to reconsider this part of the subject, and it occurred to me, that by pursuing the experiments, some results might probably appear which would be interesting in the theory of terrestrial Magnetism. I, therefore, resumed the inquiry, and have been led to some novel facts which, to me, have appeared exceedingly important, by throwing a new light on the action of caloric on Magnetism. (See the twenty-fourth Memoir.)

at twelve inches distant from its pivot. The south end of the needle was drawn towards the magnet a certain number of degrees; and this deflection being noted, the magnet was taken out of the helix and replaced again with its poles in the reverse order, by which means the north end of the needle was drawn towards the magnet, which deflection was also noted. The magnet's action on the needle being thus ascertained, the electrical force of the battery was laid on whilst the magnet was in the helix: and when the deflection arising from this combined force had been ascertained, the battery connections were reversed, and consequently the direction of the current in the helix was reversed also. This last direction of the current gave a new deflection of the needle, which, after being ascertained, was also noted down. This done, the magnet was reversed in the helix; and when the deflections of the needle arising from the current traversing the helix in each direction respectively had been ascertained, the electric current was finally cut off, and the deflecting power of the magnet alone again ascertained in the same manner as at first.

30. The bar of soft iron was next placed in the helix, and the electric current again laid on; and when the deflection arising from the polar force of the iron by the first direction of the current had been ascertained, the battery connections were reversed, and with them of course the polarity of the iron was reversed also. The new deflection was noted down, and the iron finally removed from the helix. The deflecting power of the current alone, when no iron or magnet was in the helix, was also ascertained at different times during these experiments, two sets of which were made with two different batteries—the former by an old battery and the latter by a new one. The results, with all the necessary particulars, are arranged in the following tables:—

First Series of Experiments.

Deflections with the magnet in the helix, with and without the electric current from the old battery, and magnet retouched.

With or without the current.	Marked or unmarked end of the magnet nearest the needle.	North or south end of the needle drawn towards the magnet.	Deflections.	
Without . . .	Marked	South	15°	1
Ditto	Unmarked	North	16°	2
With	Marked	South	17°	3
Current reversed.	Ditto	Ditto	7°	4
With	Unmarked	North	18°	5
Current reversed.	Ditto	Ditto	9°	6
Magnet alone .	Unmarked	North	13°	7
Ditto	Ditto	Ditto	12°	8

31. The electro-magnetic force in the helix alone, by this battery, produced no perceptible deflection of the needle; but when the soft iron was placed in the helix, the mean of several deflections, with the currents in different directions, was 17°.

32. By taking the mean of the deflections 3 and 5 in the table, which are those obtained whilst the electro-magnetic action of the current conspired with that of the magnet, and comparing that mean (17·5°) with the mean of the deflections with the soft iron (17°), we find that they are nearly to the same extent. And by comparing these again with deflections 1 and 2, which are due to the magnet alone, we discover that a current which is incapable of exalting the original deflecting power of the magnet 2°, is yet capable of raising a deflecting power in soft iron equal to the whole of that exhibited by the magnet, even when aided by the influence of the current. We discover also, by deflections 4 and 6, that the same current, when exerted in *opposition* to the energies of the magnet, is incapable of counteracting more than one-half the deflecting power of the latter. And we learn, by comparing deflections 7 and 8, which are those due to the magnet after being subjected to the *reverse* electro-magnetic action of the current, with deflections 1 and 2, that the *same* electric current which excited so great a power in soft iron was incapable of reducing the *permanent* action of the magnet more than one-fifth of that which it originally exhibited.

Second Series of Experiments.

Deflections with the magnet in the helix, with and without the electric current, with the new battery and magnet re-touched.

With or without the current.	Marked or unmarked end of the magnet nearest to the needle.	North or South end of the needle drawn towards the magnet.	Deflections.	
Without . .	Marked	South	20°	1
Ditto	Unmarked	North	19°	2
With current .	Marked	South	25°	3
Ditto reversed .	Ditto	North	1°	4
Magnet alone .	Ditto	South	11°	5
Ditto	Unmarked	North	9°	6
Magnet re-magnetized.				
Without . . .	Marked	South	21°	7
Ditto	Unmarked	North	21°	8
With current .	Unmarked	Ditto	27°	9
Ditto reversed .	Ditto	South	2°	10
Magnet alone .	Ditto	North	8°	11
Ditto	Marked	South	10°	12

With this battery the soft iron gave a deflection of 18°; and the current alone, without either magnet or iron in the helix, about 1°.

33. In this second series of experiments there is displayed a manifest superiority of electro-magnetic action over that shown by the old battery; but although deflections 4 and 10, show that the electro-magnetic action completely counterbalanced the deflecting force of the steel magnet (deflections 5, 6, 11, and 12) as obviously demonstrate that the original magnetic power was very far from being annihilated, and that, notwithstanding the vigorous electric current to which the bar had been subjected, the latter retained about one-half of its original power, which that current was unable to subdue. Indeed it appears from both series of experiments that a great portion of the Electro-Magnetism of the helix operates merely on the *disposable* part of the magnet's force, and diverts it from its original direction, in the same manner as soft iron or other magnets would do; and the electro-magnetic force thus engaged is prevented from assisting the other portion in conferring permanent effects on the steel. When the constraining electro-magnetic force is removed, the liberated disposable force of the magnet with which the former had been engaged, again resumes its original direction, and gives the needle a new deflection, in the *same direction*, though not to the same extent as at first (deflections 5, 6, 11, 12).

34. I am not aware that any one would venture to assert that electric currents, more powerful than those employed in these experiments, still existed in the steel; and if not, to what cause are we to allude the retained magnetic force? There must be some agent in operation which still sustains the polar action, and resists the energies of the assailing electric current. That agent cannot be Electricity, or it would have been subdued by the counteraction of a superior electric force; it must, therefore, be admitted, that some other physical agent, perfectly distinct from the electric, presides over the polar forces of the steel magnet.

35. I am well aware that, had the electro-magnetic force of the current been more powerful, the magnetic forces of the steel would have suffered to greater extent; and it is possible that an electro-magnetic force might be employed of sufficient extent to completely annihilate the original polarity of the steel, or even reverse its polar action. But I should wish it to be understood, that to accomplish such an effect the electric current employed must be very powerful indeed; and whatever extent of polarity might be exhibited by the steel after the removal of the exciting electro-magnetic force, the *retention* of that polarity could not be supposed to depend upon that *absent* exciter any more than the polarity of this, or any other piece of steel, could be supposed to be sustained by the absent magnet which first excited it; and our present knowledge of electro-dynamics does not permit us to indulge in the idea that any sustaining electric currents remain in the steel,

56. We have seen by the preceding experiments that the power of the magnet was considerably lessened by the action of the electro-magnetic force in the helix; but it

must be observed that the latter force had no *sustaining* power to contend with, excepting that exercised by the retention of the steel ; but if the magnet be placed under the influence of a *sustaining* magnetic force during the time it is assailed by the Electro-Magnetism of the helix, it will be found that the latter is too impotent to make any other than a very slight permanent impression on the original power of the steel magnet ; and, under some circumstances, not the slightest impression is accomplished. To prove this fact, I place the *marked* end of a magnetic bar, seventeen inches long, in contact with the *unmarked* end of the six inch cylindrical magnet whilst placed in the helix, the marked end of the latter being nearest to the needle, as represented by Fig. 10. I now transmit the electric current through the helix in a direction which tends to neutralize the Magnetism of the inclosed bar. The current is continued for more than a minute, after which it is removed, and as speedily as possible the long sustaining magnet is removed also. This done, the deflecting power of the cylindric magnet is again ascertained. The following table shows the results:—

Third Series of Experiments.

Deflections with the magnet in the helix, with or without the electric current, from a new battery.

With or without the electric current.	Marked or unmarked pole of the magnet nearest to the needle.	N. or S. end of the needle attracted.	Deflections.
Without the current . .	Unmarked	North	29°
Ditto	Marked	South	31°
Ditto	Ditto sustaining magnet attached,	Ditto	65°
With the current tending to neutralize the magnet	Ditto	Ditto	59°
Current and sustaining magnet removed . . .	Marked	Ditto	26°
	Unmarked	North	24°

37. I next place the cylindrical magnet under the influence of two sustaining magnetic bars, each seventeen inches long, submitting it at the same time to the action of an electric current tending to neutralize it. The arrangement is represented by Fig. 11, and the results were as follow:—

Fourth Series of Experiments.

	Mean Deflection of both Poles of the Needle.
Before the magnet was subjected to the action of the current . .	30°
After the magnet had been subjected to a current tending to neutralize it	31°

38. When under the sustaining force of two magnets, we find that the electric current makes no impression on the small magnet on which it operated. The trifling power which the magnet gained during the experiment was obviously due to the influence of the bars between which it was placed. The additional power given to the intervening magnet, by this means, is however but very small, never amounting to more than 2° of deflection, as I have ascertained by several experiments, by permitting the cylindrical magnet to remain between the poles of the two large ones, as in Fig. 11, for two minutes in each experiment, which is a much longer time than it remained under the same influence after the removal of the electric current in the preceding experiments. Hence, since a sustaining magnetic force may be employed to any required extent, the obvious inference is this:—*No electric current, however powerful, is capable of impairing the powers of a hard steel magnet, whilst the latter is under the protecting influence of a proper purely magnetic force.*

39. Having ascertained that the sustaining magnetic force does not operate as an exciting power (38), I was led to suppose that the power of the *protected* magnet is sustained by the mutual attractions of its own *disposable* forces (18, note) and those of the sustaining magnets—the north and south polar forces engaging with each other too intimately to be disunited by the assailing Electro-Magnetism in the helix. This view of the nature of the action led me to try soft iron as a means of sustaining the power of the magnet, whilst the latter was subjected to the action of an electric current, considering that a portion of the disposable force of the magnet would be employed by the iron, and thus be protected from the assailing electro-magnetic force; but it was found by the experiments about to be described, that soft iron affords no protection whatever to the magnet when assailed by a converse electro-magnetic force, but, on the contrary, the iron facilitates the subduction of the original powers of the steel magnet.

40. The experiments were made by placing the cylindrical magnet in the helix, and ascertaining its deflecting power on the needle at the original distance of twelve inches; then placing in contact with its remote pole a cylindrical bar of soft iron, six inches long, and about an inch in diameter: an additional deflecting force is thus given to the magnet, which deflection is also noted down. Another bar of soft iron, $3\frac{3}{4}$ inches long, and about the same thickness as the former, was next placed in contact with that pole of the magnet nearest to the needle, and the new deflection thus given

to the needle also noted down. This done, the electric current from a new battery was transmitted through the helix, whose magnetic powers were opposed to the powers of the enclosed magnet. The following table shows the results:—

Fifth Series of Experiments.

Deflections with the magnet in the helix, with and without the soft iron and electric current from a new battery.

With or without the soft iron and electric current.	Marked or unmarked end nearest to the needle.	North or south end of needle attracted.	Deflections.	
Without current or iron .	Marked	South	30°	1
With the larger piece of iron	Ditto	Ditto	42°	2
With both pieces of iron .	Ditto	Ditto	65°	3
Do. with a converse electric current . . .	Ditto	North	40° then 19°	4
Current cut off, but iron remaining	Ditto	South	25°	5
Magnet alone . . .	Ditto	Ditto	10°	6

41. The principal circumstances to be noticed in these experiments are the singular changes of polarity by the soft iron, and the final subduction of a great portion of the force of the magnet. By deflection 4, we see a transposition of polarity by the action of the current. The new deflection thus given to the needle at first rose to 40°, but gradually sank down to 19°, where it remained permanent for some time. This reduction of the deflection was of course dependent on a reduction of polar energy in the nearest piece of iron; and as the polarity of the iron depended on the polar condition of the magnet, we learn that the transient transposition of its polarity is accomplished to the greatest extent, immediately after the current has got into full play, and that it gradually subsides for about one minute afterwards, at which time it has arrived at its minimum. These versatilities in the polar action of the magnet are observable in all cases when it is subjected to a converse electro-magnetic action, whether there be any iron attached to its poles or not, though without iron they are

not so great as when that metal is present. They are exceedingly curious, and are involved in a theoretical principle which it is not necessary to enter into at present. By comparing deflections 1 and 6, we find that the magnet has lost a considerable portion of its power, which portion is greater by 6° or 8° than that usually lost when no iron is present, all other circumstances being the same; which shows that the attachment of the iron to its poles facilitates the subduction of the original powers of the magnet. (See also the first and second series of experiments.)

42. I had next recourse to the reverse process of that which was pursued in the last experiment. I placed the soft iron cylinder in the helix, and attached one pole of the cylindrical steel magnet to that extremity of it which was nearest the needle, and whilst thus arranged an electric current was transmitted through the helix. The distance beetween the pivot of the needle and nearest pole of the magnet was twelve inches. The following results were obtained:—

Sixth Series of Experiments.

	Deflections.
Magnet alone, prior to being placed in the arrangement . .	38°
Magnet attached to the iron bar, the latter being under the influence of the current	45°
Magnet alone, after the iron and current were removed . .	38°

The Magnetism of the soft iron left no additional permanent power on the steel magnet.

43. Having ascertained that an electric current is capable of subduing a considerable portion of the original power of an unprotected steel magnet (32 and 33), it became an inquiry of some interest to ascertain whether or not the same current—with the magnet reversed in the helix—was capable of restoring the power which it had previously subdued. For this purpose the cylindrical steel magnet was retouched, and after its deflecting power at the distance of eight inches had been ascertained, it was subjected to the action of an electric current from a perfectly new battery, whose copper exposed about a square foot of surface, with a proportionate round zinc cylinder inside. The battery was made exceedingly active by a solution of nitro-sulphuric acid. The following table shows the results:—

Seventh Series of Experiments.

Magnet alone,	previous to its exposure to the current . .	39°
Ditto	after being exposed to a *converse* current . .	21°
Ditto	after being exposed to a *direct* current . . .	25°
Ditto	after a second exposure to ditto . . .	26°
Ditto	after several other exposures to ditto . . .	26°

44. From this series of experiments we learn that the active electric current here employed was incapable of restoring one-third of that portion of the deflecting force of a newly magnetized hard steel bar, which it was previously enabled to subdue, although as powerful during the one process as during the other. This exceedingly curious fact I have found in the result of several other experiments, and with batteries of different powers; but the same law does not hold good unless the magnet has been magnetized to a high degree previously to its being subjected to the electric currents; nor perhaps will it be found *generally* exact even under these circumstances, although I have not met with any results in direct contradiction to it. And although the ratio of the *subdued* and *restored* force may vary, I have cause to believe that in no case will the restored force be more than one-half of that which had been subdued by the same current, when the magnet employed is hard cast steel, and not below the dimensions of that which I have described (28), and the Voltaic plates of proportionable magnitude.

45. Another interesting fact presented itself by neutralizing the cylindrical steel bar, and afterwards magnetizing it by the electro-magnetic action in the helix, whilst the latter was transmitting a copious and active current from the battery last described (28, 32), furnished with a new zinc. The deflecting power which the steel acquires, by this process, is about one-half of that which it exhibits by means of ordinary magnetic excitation. I have doubled and trebled the coil in the helix, but in no case has the magnetic power of the steel increased above that I have just mentioned. The facts developed by these experiments are partly attributable to the magnetic force receiving different forms of distribution by the magnetic and electric processes of excitement, though principally from an absolute incapacity in the latter of bringing forth those intense magnetic forces which hard steel is susceptible of displaying. There seems, indeed, to be a vigorous tension in the Magnetism of hard steel which that of electric currents cannot compete with in vanquishing those formidable resisting forces presented by hard ferruginous bodies, whilst undergoing the magnetizing process. Even the Magnetism of soft iron, when brought into play by electric currents, though much more abundant in quantity, is of far lower tension than that of hard steel. This curious fact may be shown by experiments with two horse-shoe magnets—one of which shall be soft iron, brought into play by electric currents, and the other a permanent one of hard steel. When the cross pieces of both magnets are of soft iron, the iron magnet will have the greatest lifting power; but when both cross pieces are of hard steel, the steel magnet will have the greatest; and this is the case even when the power of the iron magnet (with soft iron cross pieces) exceeds the other to a considerable extent.

46. There is a remarkable phenomenon observed whilst magnetizing hard steel by electric currents. The deflecting power of the steel is much greater whilst under the

dominion of the current than after the latter is cut off. Now, as the helix alone exhibits no action on the needle (31, 32), the experiment shows that there is a temporary disposable force excited even in hard steel which that metal does not exhibit when the exciting cause is removed. This fact probably arises from a new distribution rather than from an absolute loss of the Magnetism first excited by the current.

47. Having ascertained that the existence of electric currents is nowhere to be found in permanent steel magnets (24, 27), and also demonstrated the inadequacy of electric excitement to the production of that extent of magnetic energy in hard steel, which is susceptible of development by the ordinary process of magnetization (45), it may now be interesting to inquire how far the doctrine of *systems* of electric currents is susceptible of application in explaining the phenomena exhibited by permanent steel magnets.

48. Let N and N' Fig. 12, represent transverse sections of two cylindrical systems of electric currents, both of which are flowing in the same direction, as represented by the arrows; and let these cylinders be prolonged parallel to each other to any required distance behind the paper. Now, because of the electric currents on the adjacent sides of these cylinders running in opposite directions, in every pair of parallel sections similar to those represented on the paper, those cylinders will exhibit a repulsion for each other throughout their whole length, or from end to end, according to the principles of Electro-Magnetism. Let now the remote extremity of the cylinder, N', be turned towards the spectator, permitting the cylinder, N, to remain unmolested. Under these circumstances, the *same* extremities, N and N', of the two cylinders whose adjacent currents, in the former case, flowed in *opposite directions*, will now flow in the *same direction*, as may be understood by looking at Fig. 13, and consequently those extremities will attract each other. Again, let the arrows in Fig. 14 represent the directions of two cylindrical systems of electric currents placed at right angles to each other, as C and C': the adjacent portions of these currents flow in the same direction, and consequently will *attract* each other. Now place the electro-magnetic system, C', in either of the positions represented by Fig. 15, and it is seen that the adjacent currents in C and C' now flow in opposite directions, and will consequently *repel* each other.

49. From the above illustrations we learn that the extremities of two systems of electric currents will either attract or repel each other according to the positions in which they are placed, and that they do not exhibit any specific polarity in the manner of ferruginous magnets whose attractions and repulsions have no dependence whatever upon the positions in which their extremities are placed with respect to each other, but are invariably referrible to their specific polar character. There is, indeed, a striking distinction in the distribution of the magnetic force of steel bars and that exhibited by electric conducting wires, whether the latter be in a simple strand or

coiled into any particular fashion. A conducting wire formed into a hollow helix displays but very little polarity exteriorly in the direction of its axis (31, 32), because of the inner and outer sides of the coil exerting their magnetic forces in opposite directions; but with hollow steel magnets, the polar forces of each individual extremity conspire with each other, and operate in concert upon vicinal ferruginous matter, whether previously polarized or otherwise, and in precisely the same manner as such matter is operated on by *solid* magnets. Hence it is that a polarized needle or small bar, freely suspended with its centre in the equatorial plane of a hollow steel magnet, whether *inside* or *outside* of the tube, will invariably assume one and the *same* direction; whereas, a similarly suspended needle, with reference to, and under the influence of, a hollow system of electric currents, would assume *one* direction when *within*, and the opposite direction when *without*, the system; and as this peculiarity of magnetic arrangement would attend every system of electric currents that can possibly be formed, it is just to infer that the distribution of force displayed by steel magnets, or by loadstone, cannot be imitated by any system of electric currents whatever; and *vice versa*, the exquisitely uniform arrangements of enveloping magnetic action, so beautifully displayed around electric currents, appear to be totally inimitable by any known forms of ferruginous magnetic bodies.

50. It would be an almost endless task to examine every fact that might be brought to bear, directly or indirectly, on the subject of this investigation. I have not dwelt on Electro-Magnetism to the extent I would have done had my theoretical views on that department of Electricity not been already before the public, although I have cited those electro-magnetic phenomena which appear to be the most important in the present discussion. In other departments of Electricity I have enumerated such facts as have appeared necessary to collate with purely magnetic phenomena, and having discussed them individually as I have proceeded, a retrospection would be needless in this place. The inference to be drawn from the investigation of the *facts* alone appears to me to admit neither of doubt nor equivocation, and may be thus briefly stated:—*There are no facts on record which demonstrate an identity in Electricity and Magnetism; but, on the contrary, there are many phenomena which justify the idea of their being perfectly distinct powers of nature.*

W. S.

Westmoreland Cottage, March, 1838.

ON THE DIRECT ACTION WHICH CALORIC EXERCISES ON MAGNETIC POLES.

TWENTY-FOURTH MEMOIR.

1. The cause of the phenomena of terrestrial Magnetism, notwithstanding the close application of much philosophical talent to the subject, still remains concealed within a dense cloud of mystery, penetrable perhaps only by progressive steps of development, between which long periods of time may possibly intervene. The discovery of Electro-Magnetism, however, has done much to tranquillize the minds of philosophers to one general opinion respecting the grand source of terrestrial magnetic action; and the appearance of Thermo-Electricity has given a sanction to the hypothesis which independently might never have been conferred on it. But whether the *main* phenomena of terrestrial Magnetism be due to circumflowing electric floods, or to some other grand physical cause, there are certainly some singular *minor* phenomena which may probably be traced to the influence of other natural agencies; and, independently of any electric currents which it may be supposed to excite, solar heat alone appears to me to play a very active part, and especially in the diurnal variations. I am well aware that this opinion is far from being a novel one; it is indeed the opinion of many philosophers of the present day, and although not much supported either experimentally or otherwise, I believe it to be generally acknowledged by all those who have paid a due attention to the phenomena. With the exception of the interesting experiments of M.M. Coulomb, Barlow, and Christie,* I know of none that have been devoted directly to the elucidation of magnetic phenomena under various temperatures; and although the experiments I am about to describe are of a very different description to any recorded by those philosophers, and have afforded perfectly novel results, and developed unsuspected relations of heat to Magnetism, which to me have appeared to bear directly on the subject, I cannot pretend to say what may be their aspect to others, nor how far philosophers may be disposed to apply them to the solution of the curious and hitherto intricate problem which the needle's diurnal variations have so long presented to their notice.

* Subsequently to the reading of this Memoir, I observed in Dr. Brewster's *Treatise on Magnetism* that Professor Kupffer had also made some experiments of this kind, and had ascertained some important facts similar to a few of those which I met with in this investigation. But as the mode by which I proceeded is very different to that pursued by Professor Kupffer, and as several of the facts which have been developed by my experiments have not been met with by that philosopher, I have not deemed it necessary to alter the original form of this Memoir.

2. The experiments, as has already been stated (note to page 421), originated in a search for thermo-electric currents in a permanent steel magnet, and they were carried on in the following manner:—A magnetic needle, four inches long, and furnished with an agate cap, supported on a fine steel point in the centre of a graduated circle, was placed on a firm table, and the meridian line of the card properly adjusted to the vertical plane passing through the poles of the needle. In an horizontal plane, four inches higher than that in which the needle was situated, and twelve inches eastward of the needle's pivot, was placed a flat bar magnet of steel. The dimensions of the bar are eight inches long by one inch broad, and about a quarter of an inch thick. It was well hardened and magnetized, and when placed nearly parallel to the magnetic meridian, having its centre opposite to the pivot of the needle, and its marked pole southward, the south end of the needle was drawn towards the magnet $2\frac{1}{2}°$; a position selected for it to stand in until the heat of a spirit lamp should be applied to the magnet.*

3. The needle being thus adjusted and perfectly at rest, the flame of a spirit lamp was placed directly under the marked end of the magnet, and in such a position as it might heat the polar point, which is always situated at some distance (half an inch or more in some magnets) from the extremity of the bar, to the greatest extent. In a very short time the needle was observed to move in consequence of the heat varying the magnetic action of the bar; but instead of its south end, which was nearest to the heated pole of the magnet, receding to the meridian, as I had been led to expect, it soon showed its tendency to move the other way, and to increase the angle of deflection. This novel result led me to continue the heat for some considerable time, and until the heated end of the magnet had passed through all the shades of colour from that of pale straw to pale blue, and eventually the needle marked an angle of 6·5°, having gained 4° of deflection by the heating process. The lamp was now removed from the magnet, and in one minute afterwards the needle was observed to recede again, and the angle progressively diminished as the bar became of a more equable temperature throughout; and when the general temperature had again subsided to the original standard, the needle rested at a deflection of 4°, being 1·5° more than the original angle prior to the application of the lamp. This experiment was repeated by again placing the lamp under the marked end of the maguet, until the needle again marked an angle of 6·5°. When the magnet had cooled to its first temperature, the angle of deflection was again 4°. Hence the results in both experiments were precisely the same; at least the *extreme* and *terminal* angles of deflection were respectively the same as the extreme and terminal angles in the former experiment.

* This position of the needle was afterwards found to be of no particular use; and, after a few preliminary experiments, the *magnetic meridian* was chosen for the first position of the needle in each experiment of this kind.

4. The lamp was now placed under the *unmarked* pole of the magnet, and shortly afterwards the needle was observed to move towards the meridian line of the card. In about ten minutes the deflection was 0°, but the needle could not be made to pass that point, although the lamp was kept under the magnetic pole for fifteen minutes, at the expiration of which time it was removed. The needle, shortly after the removal of the lamp, began to return to its previous deflection; and when the magnet had been restored to its original temperature, the needle came to rest at 2·5°, or precisely in its original position prior to the application of heat in the first experiment.

5. The results of the above experiments will be more conspicuously exhibited in the following tabulated form. The deflections are given for the north end of the needle:—

First Experiment.

The magnet 4 inches above the level of the needle, parallel to it, and its centre 12 inches to the eastward of the needle's pivot. (See the whole arrangement in Fig. 1, Plate XVII.) The dotted needle is the new position taken by the compass needle when the lamp is applied to the marked pole of the magnet.

	Deflections.		
Before the magnet was heated, . .	2·5° W	Transient difference	= 4·0°
Marked pole heated, ...	6·5 W		
When the magnet had returned to its original equable temperatue, . .	4·0° W	Permanent difference	= 1·5°
Original deflection, . . .	2·5 W		

Second Experiment. (*See Fig.* 2.)

The situation of the magnet as in the first experiment.

	Deflections.		
Before the magnet was heated, . .	4·0° W	Transient difference	= 4·0°
Unmarked pole heated, ...	0·0 W		
When the magnet had returned to its original equable temperature, . .	2·5° W	Permanent difference	= 1·5°
Original deflection, . . .	4·0 W		

6. There is something singularly uniform in the results of these two experiments, from which it would appear that, when the original magnetic force is disturbed by an augmentation of temperature at one extremity of the bar much above that of the other, by any quantity, F, the resulting *permanent* force, f, will always be in a certain ratio with it. In both the above experiments, if we assume the deflections of the needle to be due to the introduction of a new force into the magnet, the *transient* new force, F, is to the resulting *permanent* new force, f, as 4 : 1·5, or at least as nearly so as those arcs are to their sines, which in this case would be a tolerable approximation.

Now, from whatever cause the variation in the magnetic intensity may have immediately proceeded, it obviously originated from the agency of caloric as a disturbing force, whose action on the magnet, even by a diversity of effects, might, directly or indirectly, be productive of the phenomena exhibited by the needle. For instance, the phenomena might obviously be explained by admitting any of the following effects being produced in the magnet by the action of caloric:—By an augmentation of power in the heated pole; by a diminution of power in the cooler pole; by an augmentation of power in both poles, but to the greatest extent in the heated one; by a diminution of power in both poles, but greatest in the cooler one; by a change of position in the bar of either north or south pole, or both of them. Under any of these circumstances the deflections of the needle might obviously have occurred; and perhaps there are some others which the heat might have occasioned in the magnet that would have been productive of similar deflections. I thought also of the effects which thermo-electric currents might produce on the needle, provided their existence could be admitted; and although the permanent effects which were left on the magnet could not be easily reconciled to their action, the *transient* deflections, having an obvious dependence on the point of heat, required further experimental investigation to ascertain whether the presence of thermo-electric currents were or were not the cause. For this purpose three other stations were selected for the magnet, and experiments similar to those already described were made at each station. The magnet was brought closer to the needle, in expectation of the effects being increased. The results are arranged in the following tables:—

Third Experiment. (*See Fig.* 1.)

The magnet re-magnetized 4 inches below the level of the needle, and 6 inches to the east of it. Marked pole southward.

	Deflections.		
Before the magnet was heated, . .	2·0° W	Transient difference	= 8·5°
Marked pole heated, ...	10·5 W		
When the magnet had returned to its original equable temperature, . .	6·0° W	Permanent difference	= 4°
Original deflection, . . .	2·0 W		

Fourth Experiment. (*See Fig.* 2.)

The situation of the magnet as in the third experiment.

	Deflections.		
Before the magnet was heated, . .	6·0° W	Transient difference	= 8·5°
Unmarked pole heated, ...	2·5 E		
When the magnet had returned to its original equable temperature, . .	2·0° W	Permanent difference	= 4°
Original deflection, . . .	6·0 W		

Fifth Experiment. (*See Fig.* 3.)

Magnet nearly parallel to the magnetic meridian, 4 inches *below* the level of the needle, and 6 inches westward of it. Marked pole of the magnet southward. Its force was that left by the last experiment.

	Deflections.		
Before the magnet was heated, . . .	2·0° W	Transient difference	= 4·5°
Marked pole heated, ...	2·5 E		
When the magnet had returned to its original equable temperature, . .	1·75° W	Permanent difference	= 0·25°
Original deflection, ...	2·0 W		

Sixth Experiment. (*See Fig.* 4.)

The situation of the magnet the same as in the fifth experiment. Not re-magnetized.

	Deflections.		
Before the magnet was heated, . .	1·75° W	Transient difference	= 3·25°
Unmarked pole heated, ...	5·5 W		
When the magnet had returned to its original equable temperature, . .	1·75° W	Permanent difference	= 0″
Original deflection, ...	1·75 W		

7. The results of the fifth and sixth experiments being so very different to any of the preceding ones, it occurred to me that this difference must either arise from the new position of the magnet (west instead of east of the compass needle) or from a want of re-magnetizing. Accordingly these experiments were repeated under precisely the same circumstances as before, with the exception of the magnet being re-magnetized by the application of a horse-shoe magnet. The following are the results of a repetition of the fifth and sixth experiments:—

Fifth Experiment repeated. (*See Fig.* 3.)

Magnet re-magnetized.

	Deflections.		
Before the magnet was heated, . . .	2° W	Transient difference	= 8·75°
Marked pole heated, ...	6·75 E		
When the magnet had returned to its original equable temperature, . .	2·25° E	Permanent difference	= 4·25°
Original deflection, . . .	2 W		

Sixth Experiment repeated. (*See Fig.* 4.)

Magnet not altered from last experiment.

	Deflections.		
Before the magnet was heated, . .	2·25° E	Transient difference	= 8·25°
Unmarked pole heated, ...	6 W		
When the magnet had returned to its original equable temperature, . .	1·75° E	Permanent difference	= 4°
Original deflection, . . .	2·25 E		

Seventh Experiment. (*See Fig.* 3.)

Magnet retouched. Situated 4 inches above the needle and 6 inches westward of it. Marked pole southward.

	Deflections.		
Before the magnet was heated, . .	2° W	Transient difference.	= 7·5°
Marked end heated, ...	5·5 E		
When the magnet had returned to its original equable temperature, . .	1.75° E	Permanent difference	= 3·75°
Original deflection, . . .	2 E		

Eighth Experiment. (*See Fig.* 4.)

Magnet not molested since seventh experiment.

	Deflections.		
Before the magnet was heated, . .	1·75° E	Transient difference	= 7·25°
Unmarked pole heated, ...	5·5 W		
When the magnet had returned to its original equable temperature, . .	1·75° W	Permanent difference	= 3·5°
Original deflection, . . .	1·75 E		

8. The uniformity which the results of these experiments have manifested is so remarkably exact as almost to surpass belief; and I have myself really been astonished at the rigid accuracy in the corresponding phenomena, as they were successively developed, and over the display of which I was soon led to discover I had no possible control, when the circumstances of the experiments were strictly the same. The transient deflections which appear in the tables were the greatest I could obtain by the means employed. They invariably increased most rapidly within the first seven minutes after the application of the spirit lamp, and generally attained a *maximum*

before the expiration of twelve minutes. A longer continuation of heat would sometimes cause an increase of a quarter of a degree, but hardly ever more; and in no instance could I get an increase of deflection after the expiration of fifteen minutes, although on some occasions the lamp was kept burning under the pole of the magnet for more than half an hour.

9. The maximum and minimum deflections due to heat in the last six experiments were so obviously near to the ratio of 2 : 1, throughout the whole series, that one would be led to imagine that such is the absolute ratio of the maximum or minimum disturbing forces, or of the changes which took place in the magnet by the action of caloric. Hence, by again representing those forces by F and f, we have

$$F : f :: 2 : 1 \quad \text{or } F = 2f,$$

which is the remarkably simple law developed by these experiments.

10. It is true that the first results of the fifth and sixth experiments are not conformable to that law, but it must be remembered that they were not made under the same circumstances as the rest. The magnet had not been retouched since the fourth experiment, and consequently was still under the effects, whatever they were, of the process of the two preceding experiments; and it cannot but be interesting to observe the remarkable correspondence of the results of these experiments, when repeated with the magnet retouched, with those of all the rest.

11. The first results of the fifth and sixth experiments, though not in accordance with the same law which is so obviously displayed by the others, are not without a due share of interest. They show that there may be a possibility of completely extinguishing these *permanent* effects of heat upon the magnet which are so conspicuously manifested by the other experiments; and they prove to demonstration that the *transient* effects may be considerably abated by renewed applications of heat to the magnet. But at this stage of the inquiry the phenomena seemed to admit of such a diversity of causes for their explication; and so many thoughts flashed across the mind respecting the most eligible mode of pursuit, that it was extremely difficult to choose that experiment which ought to be made first, in preference to any other, for the purpose of arriving the most speedily at conclusive results. The point which appeared most easy to decide was that respecting electric currents, which might be supposed to be generated by the action of the applied heat; but if any such currents have existed, the phenomena hitherto exhibited by the compass needle have not given any indications of that regularity of their magnetic actions as ought to have been expected from the known laws of Electro-Magnetism—unless, indeed, they have taken very different routes to any previously developed by heat in the simple metals;* for it is obvious that the deflections of the needle were in the *same direction*, with respect to the point of heat on the magnet, whether the latter was *above* or *below* the level of the needle,

* See my paper on the Thermo-Electricity of simple bodies, second Memoir.

an occurrence not likely to have taken place from the influence of electric currents. Moreover, the *quantity* of action at the distance (6 inches) between the magnet and needle was much greater than any known thermo-electric current could produce; and the residual *permanent* action could not easily be reconciled to the operation of any such agency. However, to decide this point still more effectually, the magnet was placed on the same horizontal level as the needle, and at the same distance (6 inches) from it as before; first on the west and then on the east side of the needle—heat being applied at each station, as in the preceding experiments; for it was obvious that if the deflections were due to a *direct* action of caloric on the polar forces of the magnet, independently of an intermediate agency of electric currents, the horizontal action on the needle would be greater than when the magnet was either *above* or *below* that level of the needle. The magnet was retouched and placed parallel to the needle. The following are the results.

Ninth Experiment. (*See Fig.* 3.)

Magnet retouched, and placed 6 inches westward from, and parallel to, the needle. Marked pole southward.

	Deflections.		
Before the magnet was heated, . . .	0°	Transient difference	= 11·5°
Marked pole heated, ...	11·5 E		
When the magnet had returned to its original equable temperature, . .	6·5° E	Permanent difference	= 6·5°
Original deflection, ...	0		

Tenth Experiment. (*See Fig.* 4.)

Magnet not molested since the last experiment.

	Deflections.		
Before the magnet was heated, . .	6·5° E	Transient difference	= 12°
Unmaked pole heated, ...	5·5 W		
When the magnet had returned to its original equable temperature, . .	1·5° E	Permanent difference	= 5°
Original deflection, ...	6·5 E		

Eleventh Experiment. (*See Fig.* 1.)

The magnet retouched, and placed in the same horizontal plane as the needle, parallel to it, and 6 inches eastward. Marked pole southward.

	Deflections.		
Before the magnet was heated, . . .	0°	Transient difference	= 12·75°
Marked pole heated, ...	12·75 W		
When the magnet had returned to its original equable temperature, . .	6·5° W	Permanent difference	= 6·5
Original deflection, ...	0		

Twelfth Experiment. (*See Fig.* 2.)

Magnet not molested since the eleventh experiment.

	Deflections.		
Before the magnet was heated, . .	6·5° W	Transient difference	= 12·5°
Unmarked pole heated, ...	6 E		
When the magnet had returned to its original equable temperature, . .	0°	Permanent difference	= 6·5°
Original deflection, ...	6·5 W		

12. The results of the four preceding experiments so effectually remove all suspicion of the agency of electric currents being concerned in the production of the phenomena that it would be totally useless to pursue that part of the inquiry any further. The whole series of phenomena obviously depend upon some *direct* action of caloric on the orginal magnetic forces. The ratio of the transient to the permanent effects, so beautifully developed in the preceding experiments, is not so precisely observed by those of the last four; but still the anomalies are not so great as to entirely overrule the probability of the general correctness of the law: indeed, the approximation to that law is too close to be otherwise considered than as favourable to its generality, as far at least as the magnet employed is concerned; but similar experiments on other magnets of different magnitudes, and other kinds of steel, require to be made before any just conclusions can be drawn respecting the generality of this or any other law connected with this branch of physics. Such a series of experiments, however, requires more time than I can afford to devote to them at present; though I cannot willingly permit so important a part of the subject to pass by without some further knowledge of it, with a view to which I have made a few other experiments, whose results appear very satisfactory, and corroborative of that law which the first series of experiments developed. The bar magnet used in the four following experiments is of cast steel, seventeen inches long, and weighs a pound and a half avoirdupoise, consequently three times the magnitude of the former (2). It was placed first westward and then eastward of the needle, six inches distant on each side, and in the same horizontal plane, having its marked end southward in both stations, and two experiments made in each in precisely the same manner as the four preceding ones. The following were the results:—

Thirteenth Experiment. (*See Fig.* 3.)

Magnet 17 inches long, weight 24 avoirdupoise ounces, placed parallel to the needle in the same horizontal plane, having its centre 6 inches westward and marked pole southward.

	Deflections.		
Before the magnet was heated, . . .	0°	Transient difference.	=14·5°
Marked pole heated, ...	14·5 E		
When the magnet had returned to its original equable temperature, . .	6·75° E	Permanent difference.	=6·75°
Original deflection, . . .	0		

Fourteenth Memoir. (*See Fig.* 4.)

Magnet not molested since the thirteenth experiment.

	Deflections.		
Before the magnet was heated, . .	6·75° E	Transient difference.	=17.75°
Unmarked pole heated, ...	11·0 W		
When the magnet had returned to its original equable temperature, . .	3° W	Permanent difference.	=9·75°
Original deflection, . . .	6·75 E		

Fifteenth Experiment. (*See Fig.* 1.)

The magnet was retouched and placed 6 inches *eastward* of the needle, and in the same horizontal plane, as in the two preceding experiments.

	Deflections.		
Before the magnet was heated, . . .	0°	Transient difference	=16°
Marked pole heated, ...	16 W		
When the magnet had returned to its original equable temperature, . .	8·75° W	Permanent difference	=8·75
Original deflection, . . .	0		

Sixteenth Experiment. (*See Fig.* 2.)

Magnet not molested since the fifteenth experiment.

	Deflections.		
Before the magnet was heated, . .	8·75° W	Transient difference	=17·75°
Unmarked pole heated, ...	9·0 E		
When the magnet had returned to its original equable temperature, . .	0·75° E	Permanent difference	=9.5°
Original deflection, . . .	8·75 W		

13. It will be observed in the last three experiments that the resulting *permanent* deflection is something more than one-half of the transient deflection, but that in the preceding one (13th) it is something less than one-half; but in no case does there appear an aberration of more than three-quarters of a degree, and as this is some-

times on one side and sometimes on the other, the formula $F = 2f$ appears to be the nearest approximation to the true law that these experiments are susceptable of communicating. If indeed we take the sines of the angles as the representatives of the new deflecting forces, the ratio of the maximum to the minimum would be somewhat different. By taking the means of the sines of the angles of deflection due to the new condition of force, with the large magnet, the formula would become

$$F = 2f - \frac{f}{8{\cdot}35} \quad . \; . \; . \; . \; b;$$

and by proceeding in a similar manner with the means of the sines of the angles developed by 9th, 10th, 11th, and 12th Experiments, with the smaller magnet, the formula becomes

$$F = 2f - \frac{f}{46{\cdot}78} \quad . \; . \; . \; . \; c.$$

14. In both these cases the resulting permanent forces are somewhat greater than one-half of the maximum transient forces; but as the excess above one-half is so very trifling a quantity, especially for the smaller magnet, which amounts to only $\frac{1}{47}$ of the minimum force, it may fairly and conveniently be neglected, being allowable for probable errors of observation; and although the formula, *b*, exhibits a greater aberration than formula, *c*, from the first formula, *a*, the latter does not appear to be very far from the truth when errors of experiment are allowed for. These formulæ, however, are of no farther use than that of exhibiting a singular coincidence in the various experiments of the new conditions of the deflecting magnetic forces, having no reference whatever to the cause of them.

15. I have made a few experiments with very small magnets, whose results have deviated but very little from the general law already pointed out (10, 14). I will state the results of two of these experiments, which are a fair specimen of all the rest. The magnet was of cast steel, five inches long, and weighed about one ounce and a quarter. It was newly magnetized and placed in the same horizontal plane as the needle, parallel to it, and four inches westward, with its marked pole southward.

Seventeenth Experiment. (*See Fig.* 3.)

	Deflections.		
Before the magnet was heated, . . .	0°	Transient difference	=3·0°
Marked pole heated, ...	3 E		
When the magnet had returned to its original equable temperature, . .	1·5° E	Permanent difference	=0·25°
Original deflection, . . .	0		

Eighteenth Experiment. (*See Fig.* 4.)

Magnet not molested since last experiment.

	Deflections.		
Before the magnet was heated, . .	1·5° E	Transient difference	= 3·5°
Unmarked pole heated, ...	2 W		
When the magnet had returned to its original equable temperature, . .	0·25° W	Permanent difference	= 1·75°
Original deflection, ...	1·5 E		

16. In the experiments with the small five-inch magnet, the maximum deflection was usually attained in three or four minutes, the needle never advancing after the fourth minute; but, on the other hand, it would sometimes return half a degree or more; and as I have never yet seen an instance of the deflection increasing after such retrograde movement, it is a good indication of the angle having attained its maximum. The maximum deflection with the large eighteen-inch magnet was attained usually in twenty-five minutes: forty minutes' duration of heat never increased the deflection above that attained in twenty-five, nor lessened it more than half a degree. With the small five-inch magnet, ten minutes' continuation of the lamp would sometimes reduce the maximum angle 2° or more; but in no instance have I seen the needle return to its first position during the application of heat, but it has sometimes stood, after the magnet had returned to its original equable temperature, at the same angle as the lamp left it. Hence the results which are so uniformly exhibited by large magnets, are not to be obtained in very small ones, unless the lamp be removed at the precise moment of maximum deflection, under which circumstances the phenomena appear to be regulated by the same laws in all those hitherto tried; and probably in all other bar magnets of the usual shape which weigh more than two ounces, and treated in the manner I have described—that is, by placing the lamp under the poles, and allowing the flame to play close to the ends of the bar during the whole time of its application.

17. The laws which I derive from these, and other similar experiments,* are simply as follow:—

First Law.—The pole of each magnet which receives caloric acquires an ascendency of deflecting force to a certain amount over the other pole, which ascendency is at a maximum during the supply of caloric.

* It would be useless to detail the whole of the experiments which I have made with a view to test the accuracy of the law, since those which I have detailed appear sufficient to establish it, and that I have met with none that have militated against it. I may just mention that I have made many experiments with the same magnets, with their poles placed in the reverse order, so that the marked and unmarked poles of the magnet were respectively opposite the marked and unmarked poles of the needle—in which case the deflections of the latter were the effects of repulsion. The ratio of maximum and minimum deflection was about the same as that developed by the attractions in the detailed experiments.

Second Law.—When the supply of caloric ceases, the ascendency of force acquired by the receiving pole lessens, and eventually subsides to about one-half of the maximum ascendency; and retains the latter ascendency until some extrinsic agent again disturbs the polar forces of the magnet.

18. There seems to be a capriciousness in the phenomena exhibited by a thin strip of steel, such as a lady's busk, not discoverable in thicker pieces. The busk which I employed is fourteen inches long and one-and-a-half inches broad. When placed parallel to the needle, at the distance of six inches either eastward or westward, the heated pole acquired an ascendency of deflecting force, agreeable to the first law, for the first minute, or a little more; but, by continuing the heat for a minute or two more, the needle would sometimes make an excursion the other way, and it would be difficult to predict at what angle it would settle when the busk had become cooled again. Frequent heating of the poles, however, without retouching the steel, brought the phenomena to a more uniform order.

19. I have already stated (6) that there appeared to be several ways of explaining the phenomena according to the effects which heat might produce in the magnet; and having now determined the non-interference of electric currents, I next endeavoured to ascertain in what manner each individual pole of the magnet affected the needle: for this purpose the magnet was placed at right angles to the magnetic meridian, having its nearest extremity six inches westward from the pivot of the needle, and sufficiently southward or northward of the magnetic equator of the needle to hold the north or south end of the latter due west. When the *unmarked* pole of the magnet was nearest the needle, the north end of the latter was, of course, brought west; and when the *marked* pole of the magnet was nearest the south end of the needle, was brought west. The following tables show the results:—

Nineteenth Experiment. (See Fig. 5.)

Magnet retouched; in the same horizontal plane as the needle; having its *unmarked* pole nearest to, and six inches westward of, the needle. Observe, the deflections are registered for the north end of the needle.

	Deflections.		
Before the magnet was heated, . .	90° W	Transient loss sustained by heat	= 21°
Unmarked (nearest) pole heated, .	69 W		
When the magnet had returned to its original equable temperature,	84° W	Permanent loss sustained by heat	= 6°
Original deflection, ...	90 W		

Twentieth Experiment. (*See Fig.* 5.)

Magnet retouched and placed as in the nineteenth experiment.

	Deflections.		
Before the magnet was heated, . .	90° W	Transient loss sustained by heat	= 8°
Marked (remote) pole heated, . .	82 W		
When the magnet had returned to its original equable temperature,	86° W	Permanent loss sustained by heat	= 4°
Original deflection, . .	90 W		

Twenty-first Experiment. (*See Fig.* 6.)

20. The magnet was retouched and placed in the same position as in the two preceding experiments, having its poles in the reverse order—viz. the *marked* pole nearest to the needle, and five inches westward of its pivot.

	Deflections.		
Before the magnet was heated, . .	90° E	Transient loss sustained by heat	= 27°
Marked (nearest) pole heated, . .	63 E		
When the magnet had returned to its original equable temperature,	80° E	Permanent loss sustained by heat	= 10°
Original deflection, . .	90 E		

Twenty-second Experiment. (*See Fig.* 6.)

The magnet was retouched and placed as in the twenty-first experiment. (Poles reversed.)

	Deflections.		
Before the magnet was heated, . .	90° E	Transient loss sustained by heat	= 9°
Unmarked (remote) pole heated, .	81 E		
When the magnet had returned to its original equable temperature,	85° E	Permanent loss sustained by heat	= 5°
Original deflection, . .	90 E		

21. The results of the nineteenth, twentieth, twenty-first, and twenty-second experiments, instead of being explanatory of the previously developed phenomena, have some tendency to involve the cause of them in still greater obscurity than before; or at any rate they have shown that the cause must be looked for in some other way. By these experiments the heated pole of the magnet invariably lost a much greater quantity of deflecting force than was lost by the unheated one; whereas, by all those experiments in which the magnet was parallel to the needle (18), the heated pole as

invariably gained an ascendency of deflecting power. Now the latter effect could not happen by the heated pole losing more power than the cool one, unless both moved from the point of heat; therefore the whole mystery seemed to be involved in the following problem:—Does the loss of deflecting power (as shown by Experiments 19, 20, 21, 22) depend upon an *absolute* loss of power in the poles of the magnet, or merely upon a change of their position in the bar, by the agency of caloric?

22. The solution of this problem required a new set of experiments, and a magnetic needle of more delicacy than that hitherto used. For this purpose I formed an astatic needle, with two sewing needles placed in a stem of dried grass, as in Fig. 7. The needles were four inches apart, with their poles the reverse of each other, and the whole suspended by a delicate fibre of unspun silk, in the interior of a cylindrical glass jar, as seen in the figure. The system, having a slight directive tendency, was properly adjusted to the meridian line of a graduated card, which was secured in the jar, beneath the lower needle, which, being the more powerful of the two, was that which gave direction to the system.

23. The eight-inch magnet, being newly touched by the exciting horse-shoe, was placed at right angles to the meridian line of the card, and in the same horizontal plane as the lower needle, with its marked pole to the south side of the glass containing the system, and its unmarked pole westward, and so adjusted as to keep the lower needle in the meridian line. In this position, the south end of the needle necessarily pointed to the centre of force, or to the true pole of the marked end of the magnet. A horizontal plan of the arrangement of the magnet and lower needle is seen in Fig. 8.

Twenty-third Experiment.

24. The lamp was placed under the marked (nearest) extremity of the magnet, and in three minutes the south end of the needle deflected 5° towards the centre of the magnet, showing that the centre of force in the heated extremity had moved in that direction. The lamp was continued for a few more minutes, but the deflection did not increase accordingly. When the magnet had returned to its original equable temperature, the angle of deflection was about 2°. This latter fact showed that the pole moved back again as the magnet cooled, though it never returned to its original position.

Twenty-fourth Experiment.

25. The magnet was not molested since the last experiment, and the lamp was applied to the remote (unmarked) pole. In ten minutes the south end of the needle had

moved over 2° towards the cold extremity of the magnet—that is, it returned to the meridian line, showing that the unheated pole as decidedly as the heated one moved *from* the point of heat. When the magnet had returned to its original equable temperature, the needle still remained on the meridian line ; or if it moved at all the arc was so trifling as not to be perceptible. This singular fact induced me to repeat the experiment with the magnet retouched ; because from previous experience I was led to suppose that this circumstance would be the means of giving different results. I consequently retouched the magnet, and placed it as before with its marked pole to the south end of the needle, as in Fig. 9, and so adjusted it that the needle rested directly over the meridian line of the card. The following are the results:—

Twenty-fifth Experiment. (*Fig.* 9.)

26. The lamp was placed under the unmarked (remote) extremity of the magnet. In three minutes the south end of the needle deviated towards the east, or towards the marked extremity of the magnet, and in ten minutes the deflection amounted to about 3°, but never proceeded farther, although the lamp was continued under the unmarked end for ten minutes longer: in all twenty minutes. When the magnet had cooled to its original temperature, the needle stood 1° eastward, having receded 2°.

27. The result of this first experiment is precisely the same as that in the 24th Experiment, which showed that the centre of force in the cooler end of the magnet moved *from* the point of heat. And the recession of the needle when the magnet cooled was what was expected from the magnet being retouched previously to the application of the lamp.

28. Having now discovered that the magnetic poles are susceptible of transition from one place to another in the metal by the agency of caloric, and also the direction in which the poles move with respect to the calorific point, it appeared possible that these mutations in the positions of the poles might account for the whole of the other phenomena ; although, at first sight, the relaxation of power in Experiments 20 and 22 did not seem likely to arise from the pole advancing towards the needle, as must necessarily have been the case according to the results of Experiments 24 and 25, unless indeed the deflecting power of that pole had absolutely diminished at the same time. But if we take into consideration that the needle was under the influence of *both* poles of the magnet, instead of assuming the deflection to depend on the vicinal pole only, we are still furnished with sufficient data to account for the diminution of deflection even in Experiments 20 and 22.

29. *Explanation of the preceding phenomena upon the supposition of a motion of the poles of the Magnet, by the Agency of Calorific Matter, as demonstrated by Experiments* 23, 24, *and* 25.

It has been shown by Experiments 23, 24, and 25, that the poles of the magnet move *from* the point of heat; and by applying these polar motions to all the experiments represented by Fgs. 1, 2, 3, and 4, we shall find that the deflections of the needle would be such as are there represented. If, for instance, the magnet be so adjusted in any of those figures as to permit the needle to rest in the meridian prior to he application of heat, then a slight motion of the magnet northward in Figs. 1 and 3, or southward in Figs. 2 and 4, will cause deflections such as are represented by the dotted needles in these figures respectively. Now, instead of moving the magnets to produce the deflections, let the lamp be applied, as shown in the figures; and the motions of the poles by the agency of heat correspond in direction with the motions of the magnet, and the deflections by one of these means correspond with those accomplished by the other.

30. With respect to the phenomenon exhibited by Experiment 23, Fig. 5, when the lamp is applied to the unmarked pole of the magnet, as both poles recede *from* the compass needle, their power on it necessarily abates, and the earth's Magnetism pulls the needle from its east and west position. But when the lamp is applied to the marked or remote pole of the magnet (Experiment 24), both poles would advance towards the needle. Still, however, the experiment shows that the power of the magnet on the needle has abated, which seems contrary to what one would have expected. The phenomenon might be explained, however, by supposing that the heated pole advanced towards the needle in a greater ratio than the cool pole, by which means it would partly neutralize the effect of the latter on the needle; or by supposing that the total power of the magnet was abated by the heating process; or, which is the most probable of all, by both these changes taking place in the magnet by the agency of caloric.

31. It appears upon the whole that the polar motions in the magnet are the first grand productions by the agency of caloric; and that, independently of any absolute change in the intensity, the whole of the phenomena which I have described may be traceable to these polar motions; and, perhaps, there may be many other magnetic phenomena, both natural and artificial,* which are attributable to the same *secondary* cause, which is itself an effect of the primitive action of caloric. But as the sun's heat is constantly exerted *between* the magnetic poles of the earth, and not exterior to

* Mr. Barlow's interesting experiments on the magnetic action of heated iron, occasionally gave very extraordinary results which that gentleman could not easily account for. *Barlow's Magnetic Attractions*, second edition, page 142 to 149.

Mr. Christie also met with some curious anomalies in his valuable experiments on this subject. *Philosophical Transactions* for 1823, 1825, 1826. But whether the facts which I have discovered are calculated to throw any new light on the anomalies which attended these gentlemen's experiments, I am not prepared to say.

them, it will be necessary to ascertain in what manner the magnetic poles are affected by placing the point of heat *between* them, before the phenomena can be applied to the explication of terrestrial magnetic mutations. The following experiments were made for this purpose:—

Twenty-sixth Experiment.

32. The eight-inch magnet (2) was retouched and placed horizontally at right angles to the magnetic meridian, having a needle arranged at each pole, as in Fig. 10, directly opposite to the centres of magnetic force. The lamp was placed directly under the centre of the magnet. In one minute both needles began to move, and in five minutes each needle had attained a deflection of 5° or thereabouts. The deflections were in the directions represented by the dotted needles, showing that both poles of the magnet had moved *outwards*, or, as in the previous experiments, *from* the point of heat.*

33. This important result in connection with those results obtained by Experiments 23, 24, and 25, appear to develop a certain determinate action which caloric exercises on the poles of the magnet, viz.—That the magnetic poles move *from* the point of heat; or in general, that *the magnetic poles move in the direction of the calorific current.* Should this law become established by future experiments, and that it can be proved experimentally that a current of caloric will move the magnetic poles *laterally* as well as in the direction of their axes, there would be little difficulty in accounting for the revolutions of the terrestrial magnetic poles in their respective latitudes; and I have no doubt, from the results of some experiments that I have made with flat pieces of steel, that the magnetic poles of the earth are susceptible of a *lateral translation* by the direct action of solar heat alone; and that by means of a magnetized steel globe and a spirit lamp, I can readily suppose that the revolutions of the terrestrial magnetic poles might be very beautifully imitated. But I have not, at present, any more spare time to devote to this interesting subject; I must, therefore, content myself, till some more favourable opportunity presents itself, with having called the attention of philosophers to this novel mode of investigation, being perfectly aware that there yet remains a rich harvest for those who may venture on the pursuit. In conclusion, I would beg permission to state that the expansions and contractions of the magnetic axis, as shown by Experiments 23, 24, 25 and 26, appear to me to afford sufficient data for supposing that the terrestrial magnetic axis suffers similar mutations by the direct action of the sun, and that the phenomena of diurnal variation, and change of intensity on the needle, are probably traceable to these *secondary causes.*

* These results, which are so very different to any hitherto made known, cannot be accounted for upon any supposed motions of the neutral plane of the magnet, in the manner which has been attempted to explain some of the interesting phenomena discovered by MM. Coulomb and Kupffer.

Should I be correct in my conjectures, there is not only a cause of *translation* of the terrestrial magnetic poles, but also a cause of *sustentation.* The sun's heat may possibly be the *primitive cause* of both—*directly* the cause of *translation ;* and, through the intermediate agency of Electricity, indirectly the cause of *sustentation.*

W. S.

London, December, 1838.

ON SOME PECULIARITIES IN THE MAGNETISM OF FERRUGINOUS BODIES.

TWENTY-FIFTH MEMOIR.*

PART I.

(Read before the Manchester Literary and Philosophical Society, Nov. 29th, 1842.)

1. Some of the various topics that I shall have to notice in this Memoir have previously been touched on, at different times, by several philosophers eminent in this branch of research, each of whom has produced data conducing more or less to the framing of a true theory of Magnetism.

2. From the time that the diurnal vicissitudes of terrestrial magnetic action on the compass needle were made known by Celcius and Graham, about the year 1722,† the attention of philosophers has been directed to this curious subject; and the observations and experiments of Canton, about the year 1758,‡ gave rise to the idea that the diurnal variations of the needle have a considerable dependance on the vicissitudes of the temperature of the globe, which, if exteriorily produced, must necessarily be associated with the solar calorific rays.

3. Canton's observations were corroborative of those of Graham, who found that the daily *westward* excursions of the needle commenced between eight and nine in the morning, and arrived at a maximum about two in the afternoon ; thence receding till next morning, when the needle recommenced its westward advance.

4. Canton's extensive observations, which were carried on throughout every season of the year, enabled him to discover that these diurnal excursions of the needle were least of all in the winter season, and that they gradually increased through the spring months, until the later part of June, in correspondence with the advances of the sun from the southern to the northern tropic; that in June the daily variation was a maximum, and that it again diminished with the recession of the sun till the latter part of December.

* Manchester Memoirs. † Philosophical Transactions, Abridged, vol. vii. 27. ‡ Ibid. vol. xi. 241.

5. From the data thus collected, Canton was led to infer that *solar heat* is probably the chief agent concerned in the production of the daily variation; and he devised a very ingenious experiment to illustrate his views, which, however, it failed to do strictly, for in that experiment he employed magnets uniformly heated, instead of imitating in them the unequal distribution of heat on the surface of the earth.*

6. At a subsequent period Canton made an extensive series of experiments on magnetic needles, by vibrating them at different degrees of temperature, from which he found that an elevation of temperature invariably produced a diminution of power.

7. Professor Christie arrived at similar results by vibrating magnetic needles, alternately exposed to, and screened from, the sun's rays.†

8. Hence it has become an established fact, that a steel magnet, whether large or small, loses some portion of its power by an elevation of temperature, whether heated by the sun's rays or by other means.

9. The experiments of Canton, however, have a still higher interest, by showing that the heat of boiling water not only produces a loss of power, but that a portion of that loss is permanent; so that the magnet never recovers its original power by the mere act of cooling to the original temperature.

10. I have repeated Canton's experiments, and have found the same results; and have discovered also, that the loss of power, both transient and permanent, varies considerably under certain circumstances, and have met with other curious results, which will be made known to this Society in another communication.

11. From the time of Canton's discoveries of the action of heat on magnetic bodies till the year 1819, philosophers had been led to view the influence of any elevation of temperature as detrimental to the full display of the magnetic powers of ferruginous bodies; and although the idea amounts to a demonstrable fact, as far as previously magnetized steel and loadstone are concerned, Professor Barlow showed, by some highly interesting experiments, conducted with great care, about the period last named, that ferruginous bodies generally, whether in the character of steel or iron, when under the influence of terrestrial magnetic forces only, acquire a greater action on the compass needle when at a blood-red heat than at any other temperature; but that at a full white heat the magnetic action entirely disappears.‡

12. The discovery of Electro-Magnetism by Professor Œrsted, in 1819, gave M. Ampere an opportunity of supposing that terrestrial Magnetism might possibly be due to electric currents circumflowing the body of the globe; but it was not till the

* "Canton placed a compass needle under the influence of two magnets, one on each side. When boiling water was poured on one magnet, the action of the other predominated on the compass needle. He also exposed one of the magnets to the rays of the sun, whilst the other was screened, and found that the heated magnet lost a portion of its power."—*Phil. Trans.*

† Philosophical Transactions.

‡ Barlow's Magnetic Attractions and Philosophical Transactions.

discovery of Thermo-Electricity, by Dr. Seebeck, in 1822, that any idea could be formed of the *source* of those currents which the hypothesis of Ampere required.*

13. Seebeck's discovery, however, tended to establish a probability, at least, that such currents are in operation; and although that discovery has not thrown any new light on the *direct* influence of heat on magnetic action, but only through the *mediate* agency of Electricity, it commenced a new and highly interesting era in terrestrial magnetic inquiries.†

14. In a Memoir read to the London Electrical Society, in March, 1838, I stated that I had discovered "some novel facts, which to me appeared exceedingly important by their throwing a new light on the action of caloric on Magnetism."‡

15. In the discovery of some of these facts, I was anticipated by Professor Kupffer, who, as I afterwards learned from Brewster's *Treatise on Magnetism*, had heated the extremities of a bar magnet, and thus shifted its poles. My experiments, however, were conducted differently from those of Kupffer, and led to other results than those which the Russian philosopher had discovered. They form the substance of the preceding Memoir, also read before the London Electrical Society, in the same year.§

16. In that Memoir I have shown that the action of caloric on the poles of a magnet is subject to a general law, which, as regards their transplacement *longitudinally*, or along the magnetic axis, is *from the point of heat.* In accordance with this law, the magnetic poles may be shifted either in the *same* direction, or in *opposite* directions; and the magnetic axis, joining those poles, may be either elongated or contracted at pleasure, according to the situation of the point of heat.

17. I also discovered that, after a few repetitions of an elevation of temperature, by means of a spirit lamp only, the transplacement of the magnetic poles becomes lessened; and eventually would nearly, if not totally, disappear by the same elevation of temperature; so that the magnet would require to be retouched to display the phenomenon again to advantage.

18. In the concluding paragraph of that Memoir, I summed up in the following manner:—

There appears to be "a certain determinate action which caloric exercises on the poles of a magnet, viz.—That the magnetic poles move *from* the point of heat; or in general, that *the magnetic poles move in the direction of the calorific current.* Should this law become established by future experiments, and if it could be proved experimentally that a current of the calorific matter will move the magnetic poles *laterally*

* Father Beccaria, about sixth years before, stated as his opinion, that terrestrial Magnetism was due to Electricity.—See his work on *Electricity.*

† Mr. Fox and Mr. Henwood have shown that Electric currents exist in the Cornwall mines.—*Annals of Electricity*, vol. i.

‡ See twenty-third Memoir, page 421, note; Transactions of the London Electrical Society; also Annals of Electricity, vol. v. page 46.

§ Annals of Electricity, &c. vol. iv.

as well as in the direction of their axis, there would be little difficulty in accounting for the revolutions of the terrestrial magnetic poles in their respective latitudes; and I have no doubt, from the results of some experiments which I have made with flat pieces of steel, that the magnetic poles of the earth are susceptible of a *lateral translation* by the direct action of solar heat alone; and by means of a magnetized steel globe and a spirit lamp, I can readily suppose that the revolutions of the terrestrial magnetic poles might be very beautifully imitated. But I have not, at present, any spare time to devote to this interesting subject; I must, therefore, content myself till some more favourable opportunity presents itself, with having called the attention of philosophers to this novel mode of investigation, being perfectly aware that there yet remains a rich harvest for those who may venture on the pursuit. In conclusion, I would beg permission to state, that the expansions and contractions of the magnetic axis, as shown by Experiments 23, 24, 25 and 26,* appear to me to afford sufficient data for supposing that the terrestrial magnetic axis suffers similar mutations by the direct action of the sun, and that the phenomena of diurnal variation, and change of intensity on the needle, are probably traceable to these secondary causes.

"Should these conjectures be correct, there is not only a cause of *translation* of the terrestrial magnetic poles, but also a cause of *sustentation*. The sun's heat may possibly be the *primitive cause* of both—*directly* the cause of *translation;* and, through the intermediate agency of Electricity, *indirectly* the cause of *sustentation*."

19. It appears from the above paragraph, that at the time of writing the twenty-fourth Memoir, I considered that the *lateral* translation of the magnetic poles was still wanting to complete the series of investigations which I had undertaken; and I have now the satisfaction of stating to this Society, that I have been successful in producing the *lateral* translation of the poles, and of observing that they travel *from* the point of heat in accordance with the same law as is observed by the *longitudinal* translations, or those in the line of the axis.

20. For the purpose of obtaining the *lateral* transplacement of the magnetic poles, by the influence of heat, it appeared necessary to employ a magnet of more than the usual breadth; and my first experiments, in this inquiry, were made on the blade of a saw previously magnetized. I was perfectly successful in the first trials, producing a *lateral* transplacement of one of the poles, to a sufficient distance from its original position, to cause a deviation of the needle amounting to 2° by the simple application of the flame of a small spirit lamp to one side of the saw, directly opposite to the pole.

21. When the lamp was removed to the other side of the magnetic pole, the latter again receded *from* the point of heat, as was obviously indicated by the retiring of the needle from its last position, which not only arrived at the meridian again, but passed over it to 1·5°.

* Annals of Electricity, Magnetism, and Chemistry, vol. iv. pages 152, 153. Also twenty-fourth Memoir, page 449.

22. The lamp was again placed in its first position (20), and the needle again indicated the lateral motion of the magnetic pole *from* the point of heat in the blade of the saw, as in the previous cases (20, 21); and by frequently placing the flame of the lamp alternately in the two positions, corresponding motions of the needle uniformly indicated that the pole in the saw blade, which occasioned those motions, travelled *from* the point of heat, though the distance passed over lessened gradually and with an apparent regularity.

23. Notwithstanding the accuracy with which these first results corresponded with the law of transplacement previously discovered in the direction of the magnetic axis,* it was necessary to ascertain its generality, if such existed, in other magnetized bodies; but, by extending these experimental inquiries, I soon met with some of the most perplexing results, which for a while seemed to overthrow all my previous reasoning on the subject; and certainly did at the time cast such a gloom over those expectations which had been occasioned by the uniform display of the phenomena whilst operating with the saw (20, 21, 22), that I despaired of reducing the anomalies that presented themselves to any law whatever: I therefore abandoned the experiments.

24. The anomalies in question were the occasional *advances* of the disturbed magnetic poles *towards* the point of heat, which were obviously at variance with the law which governs the transplacement in the direction of the axis; and at the time that I abandoned the inquiry, these occasional movements of the magnetic poles seemed to me to be totally inexplicable.

25. Some time afterwards, I ventured on the inquiry again; and, by closely pursuing the experiments through several repetitions, I eventually found that these movements of the magnetic poles *towards* the point of heat were as uniformly displayed as those in the opposite direction; and, what is very remarkable, though the causes of these apparently opposite movements of the poles are in operation during the whole period of the experiment—yet, at certain periods of the heating process, the force which urges the needle in one direction overcomes the force which urges it in the opposite one; and the needle, obeying the predominating force, moves accordingly.

26. Having now given a general outline of the principal experimental inquiries that have been undertaken with a view of discovering the *direct* influence of caloric on the Magnetism of ferruginous bodies, I will proceed somewhat minutely to a detail of those which have more recently devolved upon myself.

27. The apparatus employed in these Experiments consists of magnetized masses of steel, of various sizes and figures—a magnetic compass needle, two inches long, and a spirit lamp. The needle, *n s*, Fig. 11, Plate XVII, is first adjusted to the meridian line of the card; and when at rest, the north end of the magnetized steel, N S, is placed on the north side of it, with its axis of polarization in the plane of the

* Annals of Electricity, &c. vol. iv. Also twenty-fourth Memoir.

the magnetic meridian, and so adjusted that the pole nearest to the compass keeps the, *now inverted*, needle also in the magnetic meridian.

28. When this arrangement is completed, the flame of the spirit lamp is applied to one side of the magnetized steel plate, N S, as at *lamp* in the figure. The needle soon removes from the meridian line, indicating a corresponding movement of the adjacent magnetic pole of the plate, which will be *towards* the point of heat, or *from* it, accordingly with the following circumstances.

29. If the magnetized steel plate be thin, say about the thickness of the blade of a small hand-saw, and not more than two inches wide, the motions of the disturbed pole will generally be *from* the point of heat; but as the temper of the metal has much to do with the lateral movements of the poles, a hard temper will sometimes cause a temporary movement *towards* the point of heat, after which the polar transplacements are uniformly *from* the point of heat.

30. When the magnetized plate is hard, more than one-twentieth of an inch thick, and upwards of two inches broad, the first movement of the disturbed pole is almost sure to be *towards* the point of heat; and if the lamp be suddenly removed to the opposite side of that pole, the latter will return to its former position in the steel, pass over that portion, and again approach the point of heat, until the deflected needle indicates a pause.

31. The pole will now move no nearer to the point of heat, but will retrograde a little. If now the spirit lamp be brought to its first position, the needle again indicates a movement of the pole in the steel, which may now be either *towards* the lamp, or *from* it, according to the direction of the prevailing force.

32. When the lamp has been shifted from side to side for a few times, that force which urges the needle *towards* the point of heat becomes vanquished, after which the disturbed pole uniformly moves *from* the point of heat in strict accordance with the law which governs the longitudinal movements.

33. When the magnetized steel plate is of a soft temper, the lateral transplacement of the pole is uniformly *from* the point of heat in every piece of steel that I have yet operated on, and they have been of various dimensions, from one-thirtieth to half an inch in thickness. The latter thickness, however, does not allow of such facilities for polar transplacement as that of one-tenth of an inch and under. I had two pieces of steel made into a peculiar form by Mr. Dancer, Philosophical Instrument maker, from which I had expected very striking results: each piece is two and a half inches broad, six and a half inches long, and half an inch thick. They are got up in a very beautiful manner, but the results which I obtained from them were by no means answerable to my expectations. The metal is in fact too thick to yield to the influence of the trifling heat of a small spirit lamp, with the necessary rapidity for the accomplishment of extensive lateral transplacement of the magnetic poles.

34. These are the simple facts which have been developed by an extensive series of experiments ; and from which there appears to be the same law in the lateral transplacements (33), as in the longitudinal transplacements (16), when not interrupted by temper of the metal, &c.

35. The other class of facts (30, 31,) which do not accord with the general law, are not the *direct* productions of heat: they are traceable to a *mediate* cause, where the laws of action are as uniform as those which govern that class of phenomena which are the *immediate* productions of heat ; they at all times interfere with the display of the latter, by urging the needle in the opposite direction, which conceals a portion of the *direct* effects of heat, and sometimes smothers them altogether. The explanation of these counteracting effects being connected with other facts which have come under my notice, and facilitated by means of them, it will be well to defer that explanation until these other facts have been made known.

36. I believe it is generally known that the power of one pole of a magnet is increased by the application of a mass of iron to its other pole ; but I am not aware that the other changes which take place in the magnet by such treatment have ever been much studied. They are, however, important, and deserve as much attention as any other phenomena displayed by magnetic action. I will here describe some phenomena of this class which have come under my own observation, and they will give an idea of the interesting changes which take place in a magnet by the approach of soft iron.

37. In the first place, I may state that whatever side of a magnet be closely approached by soft iron, a portion of its force is employed on the approaching body, whilst another portion is set free, and ready to operate on other magnetized or magnetizable matter, properly situated within the sphere of its action. Hence it is that one pole will suspend more iron, or produce a greater degree of deflection on the compass needle, when a mass of iron approaches the other pole, than when no such iron is present ; and when the approaching iron actually comes into contact with the magnet, the power of the other pole is augmented to the greatest extent.

38. If, by means of a compass needle, we were to ascertain the situation of the unapproached pole, both before and after the application of the iron to the other pole, we should find that the former pole recedes *from* the extremity of the magnet, or retires nearer to the magnet's centre, by the approach of iron to the other pole.

39. Now, since the pole of a bar magnet operates more powerfully on a needle by the approach of iron to its other pole (37), whilst, at the same time, the former pole recedes from the needle (38), and consequently operates to disadvantage by an increase of distance, it appears obvious that the absolute gain of deflecting force derived from the approach of iron to the other pole is much greater than the needle is calculated to indicate. The same reasoning applies to the magnet's lifting power, which, in con-

sequence of an increase of distance of the pole from the iron lifted, does not indicate the extent of power absolutely gained; or rather of magnetic force liberated by the approach of iron to the other end of the magnet.

40. We next inquire into the changes that take place in that pole of the magnet which the iron approaches—or rather with which it is in contact—and we find that it advances upon the extremity of the bar. Hence we arrive at another general law—viz. when a mass of iron is brought into contact with a bar magnet, *exterior* to either pole, in the line of the axis, it occasions a transplacement of both poles towards itself.

41. If, instead of placing the iron in contact with that pole of the magnet which is most remote from the needle, we were to place it by the *side* of the nearest pole, the needle would indicate a lateral movement of the latter pole *towards* the iron.

42. With the assistance of these facts, developed by the attachment of soft iron to the poles of a magnet, in connection with the well-known fact that the resistance to magnetic excitement in steel is much greater at a hard than at a soft temper, we find a simple and satisfactory explanation of the apparent mystery which the lateral motions of the magnetic poles *towards* the point of heat at first involved (30, 31).

43. The first applications of the spirit lamp to hard steel tend to lower its temper, and consequently to lessen the resistance to magnetic excitement on that side of the pole to which the lamp is applied, without producing a corresponding diminution of resistance on the other side. Hence the magnet, under these circumstances, is similarly situated to a magnetic pole approached on one side by a piece of soft iron (41); the polar force will therefore move in that direction, or *towards* the point of heat. But when this resistance becomes so far diminished on every side of the pole, by the frequent application of the lamp warming and softening the steel, as to be overcome by the *direct* calorific force, the latter force prevails in producing the polar displacement, which is invariably *from* the point of heat. When the steel is soft and thin, the resistance of the metal is easily overcome, and the polar transplacement *from* the point of heat is accomplished even by the first applications of the lamp.

44. One of the peculiarities attending *longitudinal* polar transplacement by heat is, as I have shown in the twenty-fourth Memoir, a diminution of space passed over by the disturbed poles on each successive application of the lamp; so that, by frequently disturbing the poles by this process, the polar movements are much lessened. They are very conspicuous when the steel has been newly touched, but become much reduced by frequent applications of the lamp, if not occasionally retouched.

45. The *lateral* polar movements by heat are also much lessened by frequent applications of the lamp; and, in this respect, appear to be subject to the same law as the longitudinal movements—the greatest range being displayed on a newly-touched magnet, and least of all by the last application of the lamp.

46. From the above facts, we are led to infer that, if the sun's heat be the *direct* cause of diurnal variation, the excursions of the needle will lessen by time, unless there be some sustaining force in operation ; and therefore are less now than formerly. This view of the subject presents one of the most important problems in terrestrial Magnetism, especially when taken in connection with the probability of the daily westerly excursions of the needle being greater than the space passed over by the eastward retrograde movements, and thus leaving a daily increment of westward advance. According to this view, the period of revolution of the magnetic poles in their respective orbits would depend upon the magnitude of the diurnal increments ; and if these lessen by time, the periodic times of revolution would lengthen in the same ratio. Hence, since accurate chronological tables of the *extent* of the diurnal variation for a long series of years are the only data that can furnish the necessary information, no available means should be left unemployed to collect data of such essential importance.

47. I hope shortly to submit to the consideration of this Society other novel facts at present in my possession, from which I have every reason to believe that, if there be no sustaining force, the *intensity* of terrestrial magnetic action is considerably less than it was at an early period of the world.

P.S.—In the Report of the Twelfth Meeting of the British Association for the Advancement of Science, printed in 1843, is the following statement :—" It would appear (if earlier observations can be relied on) that the line of least intensity on successive meridians is travelling rapidly northwards." May not this transplacement of the poles be owing to solar heat ?

ON SOME PECULIARITIES IN THE MAGNETISM OF FERRUGINOUS BODIES.

TWENTY-FIFTH MEMOIR.

PART II.

(Read before the Manchester Literary and Philosophical Society, Dec. 27th, 1842.)

48. In the first part of this Memoir I had occasion to allude to the excellent experiments of Professor Barlow, on the magnetic action of heated iron (11). In the year 1827 I repeated those experiments ; and, with the exception of a peculiar phenomenon displayed in the Professor's inquiries, I met with similar results.

49. At that time I was also engaged in a series of inquiries in Electro-Magnetism, especially respecting the action of hollow ferruginous magnets ;* and as I succeeded

* Fifteenth Memoir.

in producing the same degree of deflecting power by *hollow* as by solid pieces of iron of the same form and dimensions, when magnetized by electric currents, it occurred to me that the magnetic action of iron tubes, at a high temperature, presented an exceedingly interesting problem, which, till then, had never been noticed.

50. The only hollow iron I then had at command consisted of a few pieces of an old muskst-barrel, which had previously been cut for the purpose of forming a Voltaic battery; and subsequently employed in the capacity of electro-magnets, in the series of inquiries then carrying on.

51. One of the pieces, about a foot in length, was heated to as high a temperature as an ordinary house fire was capable of raising it; at which temperature, when placed in the line of the dip, its Magnetism entirely disappeared, but returned as the temperature subsided, and eventually displayed a higher degree of magnetic action on a compass needle than it had shown previously to the heating process.

52. With this fact, however, the inquiry rested for that time, the other proposed experiments being laid aside to give place to the electro-magnetic inquiries then going on; and it was not till arranging the former part of this Memoir, that my attention was again directed to the Magnetism of ferruginous bodies at high temperatures.

53. As my first repetition of Professor Barlow's experiments had been undertaken on a much smaller scale than the original ones, I was desirous of repeating them, and also that with the hollow tube, on masses of iron much larger than those I had previously employed. Through the favour of Mr. Richard Roberts, to whom I made known my desire of carrying on these inquiries on a larger scale than I had any opportunity of pursuing them before, I have been enabled to make the necessary experiments under the best possible advantage; and, I am happy to say, with the most satisfactory results.

54. The experiments were made at the "Atlas Works," Manchester, belonging to Messrs. Sharp and Roberts, engineers, on Saturday, the 24th instant, in the following manner:—

55. The first series of experiments were with an iron tube two feet long, and two inches in external diameter, the thickness of the metal being about two-tenths of an inch. The tube, when under examination, was placed in the line of magnetic dip, having its lower extremity resting in a cavity, purposely made in a fire-brick, and its upper extremity leaning against a tall brick tile, placed in a vertical position for the purpose. A magnetic compass, whose needle is two inches long, was placed on the top of the tall tile, nearly on a level with the upper end of the iron tube, and six inches distant on the east side of it. Fig. 12, Plate XVII, represents the end of the brick against which the uppermost end, *i*, of the iron leaned, and on which the compass needle, *c*, was placed.

56. Prior to placing the iron tube in this position, and in the absence of all local magnetic action, the meridian line of the compass card was adjusted to the magnetic meridian of the place where the experiments were carried on.

57. When the needle had come to rest, the iron tube, whilst cold, was placed in its proper position (55), and the deflection of the needle due to the magnetic action of the upper end noted. The tube was then inverted, and again the deflection noted. By these means the magnetic action exercised on the needle, by the different ends of the hollow bar, prior to heating it, was ascertained, and the mean action of both ends easily determined.

58. The tube was next heated to whiteness (not to a welding heat), and in that condition replaced in its former position (55). The needle was carefully watched, and its indications strictly noted. In the first trials, the needle was much agitated during the time that the hot iron tube was being adjusted in its proper place, but soon afterwards came to rest at zero, indicating an entire absence of magnetic action in the iron, which did not reappear until the temperature had subsided so as to display a moderate degree of redness on the surface of the tube.

59. The needle first indicated a magnetic action in the iron tube by a slow movement of its north end towards it, but shortly afterwards its motion became rapid, until it arrived at its maximum of deflection, which occurred when the iron had fallen to a dull red heat. This maximum of deflection continued for a considerable time after the iron had cooled to blackness; and was not seen to lessen whilst the iron remained unmolested.

60. The agitation of the needle during the adjustment of the hot tube in the first experiment (58) was found to be occasioned by the comparatively cold tongs by which the upper end of the iron was held; and was avoided in all the subsequent experiments by applying the tongs to the middle part of the hot iron, under which circumstances not the slightest movement of the needle was observable until the heat of the iron had subsided to that stage of redness already described (59).

61. Whilst the experiments with the tube were going on, two solid cylindrical bars of malleable iron, of precisely the same dimensions as the tube, were prepared, for the purpose of making comparative experiments, and ascertaining the difference, if any, of magnetic action of solid and hollow iron at high temperatures. The experiments with these solid bars were pursued in precisely the same manner as those with the hollow bars, and the results were almost exactly the same as is evinced by the following tabulated arrangement of them:—

62. One end of each bar was marked A, and the other end B, for convenience of comparison and explanation.

First Experiment.

Iron Tube, cold,	End A, deflection 20° . .		Mean 21·5°
	End B, „ 23° . .		
White hot, ... zero.	Blood-red . . .		35°
Cooled to blackness, ...			35°

Second Experiment.

Iron Tube, cold,	End A, not ascertained.		
	End B, do.		
White hot, ... zero.	Blood-red . . .		35°
Cooled to blackness, ...			35°

Third Experiment.

No. 1, Solid Iron Bar, cold,	End A, deflection 35° . .		Mean 29°
	End B, „ 23° . .		
White hot, ... zero.	Blood-red . . .		35°
Cooled to blackness, ...			35°

Remark.—This bar was neutral nearly four minutes. The needle then moved gently to about 5°. Its subsequent movement was very rapid till it arrived at 35°. The end, A, which gave a deflection of 35° whilst cold, had acquired some fixed polarity by the process of being cut by a chisel from a long bar.

Fourth Experiment.

No. 2, Solid Iron Bar, cold,	End A, deflection 23° . .		Mean 22°
	End B, „ 21° . .		
White hot, ... zero.	Blood-red . . .		35°
Cooled to blackness, ...			35°

Fifth Experiment.

No. 1, Solid Iron Bar, cold,	End A, deflection 22° . .		22·25°
	End B, „ 22·5° . .		
White hot, ... zero.	Blood-red . . .		35°
Cooled to blackness, ...			35°

63. The general results of these experiments differ in no respect from those obtained by Mr. Barlow, as far as relates to the neutrality of iron when highly heated, and its acquiring a maximum of magnetic action at a certain degree of temperature. But we do not learn from that philosopher's account of his own experiments that the iron is neutral at a *bright red* heat. The results now detailed, however, have shown that the iron is perfectly neutral, even at a moderate degree of redness; and it is not till the temperature falls to a *dull* red heat that the needle indicates the slightest movement from the meridian. It first moves slowly till it arrives at about 5° of deflection, stands fixed for a moment, and then travels rapidly to nearly the maximum point.

64. The experiments with the hollow iron (51, 62), are, I believe, the only ones of the kind hitherto made. They are interesting on two accounts: they show that the Magnetism of hollow iron bars at high temperatures observes the same laws as solid bars of the same dimensions when similarly treated; and they also show that the *extent* of magnetic action, under these circumstances, is precisely the same, both in hollow and solid iron. This latter fact is in strict accordance with the results obtained by the action of cold iron, as shown by another interesting series of Mr. Barlow's experiments.*

65. During the time that one of the solid bars remained neutral, it occurred to me that there still existed a possibility of some slight magnetic action being present which could not be detected by the needle at the distance of six inches. I therefore caused the hot bar to be placed with its upper end at the distance of three inches from the needle, but still no magnetic effect was produced; nor do I think that any magnetic action would be detected even though the needle were placed close to the heated bar.

66. I believe that neither Mr. Barlow nor any other philosopher has hitherto ventured an opinion as to the cause of the *total absence* of magnetic action in ferruginous bodies when at a high temperature. Nor has any one, as far as I know, attempted to explain the cause of the exalted magnetic action which iron displays whilst cooling from that high temperature.

67. It is well known that I have for several years past entertained the idea that the electric, the magnetic, and the calorific matter are perfectly distinct elements of nature, and that each possesses attributes peculiar to itself. Moreover, I am of opinion that I have proved by the clearest experimental evidence that Electricity differs essentially both from the matter of heat and from Magnetism.† And if I do not deceive myself in the views which I take from the results of the experiments detailed in this Memoir, they are particularly favourable to the opinion which I have entertained respecting the distinction between Magnetism and caloric.

* Barlow's Magnetic Attractions.

† Transactions of the London Electrical Society. Also twenty-third and twenty-fourth Memoirs.

68. The grand axiom in all reasonings of this nature is simply this :—*No two particles of matter can occupy the same place at one and the same time.* By keeping this axiom in view, we are easily led to understand that either *additions* or *abstractions* of the calorific matter, to or from any body, disturb both the electric and magnetic equilibrium, if such previously prevailed, in that body. It is thus, in fact, that our thermo-electric and thermo-magnetic phenomena are produced; and that reciprocally calorific phenomena as decidedly emanate from magnetic and electric disturbance or excitement.

69. Now although the display of heat by magnetic excitement, and the display of Magnetism by calorific excitement, are in many cases entangled by electric agency, which plays an intermediate part in the process, it is a well established fact that additional portions of calorific matter forced into the substance of a magnet, disturb its powers by direct action; and it has been shown in the first part of this Memoir, that the magnetic poles are absolutely transplaced by the *direct* action of heat, which invariably repels these poles from itself; showing that, as heat is introduced to any particular part of the steel bar, the Magnetism of it is displaced, and partially removed from that part of the ferruginous mass.

70. Since then such a notable displacement of the magnetic poles can be accomplished by the heat of a small spirit lamp, it is but reasonable to infer that, by the introduction of a much larger quantity of the calorific matter, such as would raise the iron to a white heat, a complete and entire expulsion of the magnetic matter naturally belonging to the iron might be accomplished.

71. Such, indeed, are the theoretical views which I have taken in explanation of the whole of these, properly called Pyro-magnetic phenomena, in which Electricity is not an intermediate agent. Whether these views will meet the sanction of philosophers or not time alone must determine; but as no previous attempt has been made to show why iron is perfectly neutral at high temperatures, the hypothesis has at least the singular advantage of not being contravened by any other.

72. With respect to the iron arriving at its greatest degree of magnetic action at a low red heat, I am far from supposing that this superior display of magnetic powers is due to the *direct* influence of the calorific matter. I would rather refer it to the molecular movements of the metallic integrants of the bar, whilst contracting, and at a time when the magnetic matter is in the best possible condition for receiving polarization—viz. whilst re-entering the bar, and consequently in a state of motion.

73. That agitation of a ferruginous mass, and consequently of its contained magnetic particles, facilitates its polarization, is a fact long ago established; and to such a degree of importance has this simple fact arisen, that philosophers are now accusing it of being the chief agent in producing disruption of the iron axles of railway carriages whilst travelling at a great speed.

74. I, however, do not blame any magnetic action, as being either directly or indirectly concerned in these unfortunate occurrences; nor can I learn that any experimental results have hitherto been obtained that could give rise to the idea that there is even a possibility of Magnetism weakening the cohesive powers of iron; but, on the contrary, from some inquiries that I have made on this topic, I am not without hopes of proving that Magnetism, properly applied, may be made a powerful auxiliary in giving strength to iron bars.*

75. Although it has long been known that agitation facilitates the polarization of ferruginous bodies, when under the influence of a standard magnetic force such as that of the earth, I am not aware that any experimental inquiries have hitherto been made with a view to ascertain the *extent* of polarization that different kinds of iron and steel are susceptible of receiving by that simple process.† But, in order to ascertain how far the views I have taken respecting the polarization of ferruginous bodies, whilst cooling from high temperatures, accord with facts, I took an opportunity of making a few trials on those bars, both solid and hollow, which had been employed in the experiments already detailed.

76. When the bars had become quite cold, and after they had lain for a considerable time horizontally on the floor, they were severally held by the hand in the line of the dip; and, whilst in that position, agitated by blows from a wooden mallet. This operation being performed on an individual bar, it was afterwards placed in the same position as it was whilst hot, in the previous experiments (54, 62), the needle being east, at the distance of six inches as before. When the magnetic action of the upper end, A, of the bar had been indicated by the needle's deflection, it was inverted; and the action of the *now* upper end, B, ascertained in like manner.

77. The following are the results of the experiments in which the bars were agitated by a series of blows with a wooden mallet, whilst held loosely with their axis in the line of the magnetic dip. The magnetic action of the *upper ends* of the bars was alone ascertained.

Sixth Experiment.

No. 2, Solid Iron Bar, cold and unagitated,	End A, . .	deflection 26°	Mean, 25·5°
	End B, . .	,, 25	
Same bar agitated, end B uppermost,	End B, . .	deflection 34	Mean, 34·5°
Inverted and again agitated,	End A, . .	deflection 35°	

* It being well known that electric currents are capable of keeping together two distinct masses of iron, with a force equal to many tons, why, I would ask, should not the same power keep together the particles of a single piece of iron, with an equal force, in addition to the force of cohesion? Some experiments which I have made seem to sanction this view.

† Since this appeared in the Manchester Memoirs, I have been favoured by a letter from the Rev. Dr. Scoresby, pointing out an extensive series of experiments, by agitating iron in this manner, made by himself some years ago.

Seventh Experiment.

Hollow Iron Bar, cold and unagitated,	End A, . .	deflection 27°	Mean, 27·25°
	End B, . .	„ 27·5	
Same bar agitated, end B uppermost,	End B, . .	deflection 33	Mean, 33·5°
Inverted and again agitated,	End A, . .	deflection 34	

78. By comparing the results obtained by the simple process of agitation with those which the iron gave by cooling from a high temperature, the difference will be observed to be so inconsiderable as to show that the magnetic action due to the latter is not much superior to that due to the former process ; nor indeed at all more so than may be fairly accounted for by the molecular movements within the ferruginous masses employed (72).

79. In pursuing the views I had already taken respecting the complete expulsion of the magnetic matter by means of heat (66-71), it appeared likely that a bar of iron might be converted into a series of distinct magnets by heating it to a high temperature in several different parts, and permitting the intervening parts to remain comparatively cool, or below that temparature at which magnetic action was complete in the previously conducted experiments (55-62); and being desirous of embodying in this Memoir all the facts I could collect on this topic, I called on Mr. Fothergill, who had assisted me in all the previous experiments, about one o'clock this afternoon, to request his assistance again in procuring me a few more heats of the iron. This request was readily assented to, and the following experiments were made, at the Atlas Works, after four o'clock this evening.

Eighth Experiment.

80. The solid iron bar, No. 1, two feet long, was heated to a high degree of redness at one of its ends to nearly the middle of the bar, whilst the other part was kept comparatively cool, and afterwards still further cooled by immersion in the slake-trough. The bar thus treated was then placed in the line of the dip, as in former experiments, with its heated end uppermost, having the compass needle at the distance of six inches on the eastern side.

The hot end showed no magnetic action whatever. I now requested the smith who assisted me to raise the bar till nearly the whole of the heated part was above the compass. In this position the north end of the needle was drawn towards the bar about 25°, indicating the presence of a pole of the opposite kind.

The character and situation of this pole being ascertained, the bar was again gradually raised till its lower extremity was level with the compass. The deflection of the needle at first declined, and gradually returned to the meridian as the centre, or

nearly the centre, of the cold part was raised to its own level. As the bar ascended higher, the declination of the needle was again shown, but in the contrary direction to the former; and when the lower end of the bar had reached the level of the needle, the south end of the latter was drawn towards it about 27°.

81. The results of the above experiment (80) showed, in the most decisive manner, that the cool part of the iron was a perfect magnet, whilst the heated part was as perfectly neutral.

Ninth Experiment.

82. In this experiment the solid cylindrical iron bar, No. 2, was heated in the middle part and kept cool at both ends. In this condition, the bar, when placed in the magnetic dip, displayed four magnetic poles indicative of two distinct magnets, one at each end of the bar, whilst the intervening heated part was perfectly neutral. The poles exhibited by the cool parts of the iron were of the same character as those which would have been displayed by them had they been two distinct pieces, without the intervention of the heated part, and the heated part entirely removed.

Tenth Experiment.

83. A long iron bar was now heated to a bright redness, in two distinct parts, leaving both extremities and a portion in the middle quite cool. The bar having been placed in the line of the dip, displayed three distinct and perfect magnets at its three cold parts—each having its proper poles, as if a distinct and separate piece of iron. The heated parts were perfectly neutral.

84. In the class of experiments last described (80-83), the poles adjoining the heated parts of the iron began to advance upon those parts as they gradually cooled; so that, in Experiment 8, the moveable pole advanced to the extremity of the bar; and, in Experiments 9 and 10, the moveable poles advanced upon one another, and eventually merged their forces and were lost. Hence, just above a black heat, each had become one distinct magnet, as though it had been wholly heated; and thus it remained whilst kept undisturbed.

85. I have already adverted to a peculiar phenomenon occasionally observed by Mr. Barlow whilst carrying on his experiments on heated iron (48). The phenomenon in question was a deflection of the needle in the opposite direction to that which it would take according to the ordinary magnetic state of the iron. It was observed when the bars were at a bright red heat, and would continue some seconds before the needle returned to the meridian. The usual, or *positive* magnetic action, would then begin to be manifested, and the needle would travel rapidly to its ultimate position.*

86. I have anxiously watched for the phenomenon alluded to, during the progress of several of the experiments already detailed, but in no instance have I been able to

* Barlow's Magnetic Attractions, second edition, 1823, page 146.

detect it. Mr. Fothergill, however, to whom I made known my object, has informed me that he observed it in an experiment carried on by himself in my absence.

87. "The only explanation," says Mr. Barlow, "which seems to present itself of the cause of this anomalous action is, that the bar, cooling faster at its extremities than at its centre, one part of it becomes magnetic before the other, and hence gives rise to the irregular action above indicated. It must be acknowledged, however, that this explanation does not meet entirely all the phenomena recorded in the preceding table."*

88. The experiments last detailed in this part (80-83) appear to me to be well calculated to give a clue to the cause of the anomalies which Mr. Barlow met with; and although the idea of the bar's "cooling faster at its extremities than at its centre" did not appear to be a satisfactory explanation to that philosopher, there appears to be a strong probability, at least, that that circumstance was the sole cause; for, by such a cooling process, the bar would be converted into two distinct magnets, as in Experiment 9 (82), in which condition it would operate on the needle in correspondence with the apparent anomalies in question, which would thus be traced to the well-established laws of magnetic action.

89. Having now placed before this Society the experimental results I have arrived at, and the views I have taken in explanation of the phenomena, it only remains that I acknowledge the very handsome manner in which my inquiries have been facilitated through the kindness of Mr. Roberts, and the able assistance I received from Mr. Fothergill, in carrying on the experiments.

ON SOME PECULIARITIES IN THE MAGNETISM OF FERRUGINOUS BODIES.

TWENTY-FIFTH MEMOIR.

PART III.

(Read before the Manchester Literary and Philosophical Society, Feb. 21st, 1843.)

90. The results which I have arrived at, whilst repeating the interesting experiments on highly heated iron, first instituted by Mr. Barlow and others, already detailed in a former part of this Memoir, have shown in the most satisfactory manner that the magnetic forces of the earth, in this latitude at least, have no power whatever in rendering highly heated iron magnetic, whether that iron be in solid masses or in the shape of hollow tubes.

* The table alluded to is a table of the results of an extensive series of experiments, in many of which the anomaly in question presented itself.

91. I have already taken advantage of this important fact, as being favourable to my views respecting the entire expulsion of the magnetic matter naturally belonging to the iron, by the introduction of so great a quantity of calorific matter as was forced into the metal by the process of heating; but as the polarizing powers of terrestrial Magnetism are exceedingly feeble when compared to those which we have at command by the employment of artificial magnets and electric currents; and as it seemed possible that, although no polarity of highly-heated iron could be detected from the polarizing influence of the earth, yet a much higher polarizing power might develop some trace of magnetic action in the iron, even at the highest degree of temperature at which it could be kept in a solid state; and as no hypothesis ought to be ventured on the philosophical world which rests on doubtful or even partial experimental data, it was easy to perceive the propriety of extending the experiments for the purpose of ascertaining what results could be derived from the employment of a much more powerful polarizing force than that of terrestrial Magnetism.

92. In order to carry out these inquiries in the most satisfactory manner possible, I first ascertained the entire neutrality of a heated bar of square iron, six inches long, and 0·6 of an inch broad, when subjected to terrestrial magnetic action in the line of the dip, as in some of the experiments already detailed.

93. As it was necessary to have some idea of the magnetic forces which the iron bar, whilst cold and terrestrially magnetized, exercised on the needle, compared with that which it would display whilst under the influence of a powerful system of bar magnets employed in the investigation, I had recourse to the method of vibration.

94. The vibrations of the needle by the force of terrestrial Magnetism alone, independently of the presence of the iron, were ascertained to be nearly at the rate of one per second of time; and, in order to facilitate calculation, they were, by means of a counteracting magnet, reduced to the same standard, when under the influence of the system of bar-magnets placed horizontally in the magnetic north of the needle—the pivot of the latter being eight inches distant from the vicinal pole. The compass box was elevated on a brick, about four inches and a half above the level of the system of magnets.

95. The iron bar was now placed with one end on the system of magnets, and the other end resting on the edges of the brick, with its axis in the magnetic meridian. The vicinal end of the iron was two inches from the pivot of the needle, the north pole of which was drawn forcibly towards it.

96. Under these circumstances, the needle was made to vibrate, and the force was such as to cause about thirteen or fourteen vibrations per second. The vibrations were so rapid, however, that it was difficult to count them with precision; but by taking notice of the appearance of the needle on one side of the meridian only, the difficulty of counting is much lessened; and by this means I was enabled to ascertain

that the *double* vibrations amounted to more than six per second—sometimes there appeared to be nearly seven per second—but as no great exactness is required in cases of this kind, I have allowed six double or twelve single vibrations per second, which is a little below the real number which the needle performed whilst under the influence of the magnetic force of the iron, when thus in contact with the system of bar-magnets.

97. The magnets being removed, and the needle again at liberty, the iron was placed with its axis in the magnetic dip, having its upper end resting on the brick at the same distance from the needle as in the previous experiment. The needle was now vibrated under the dominion of both the magnetic force of the terrestrially polarized iron and that of the earth itself, and was found to perform 1·5 vibrations per second.

98. Having thus obtained the rate of vibration under these three different circumstances, it was easy to ascertain the ratio of the magnetic force exercised by the iron whilst under the influence of the steel bars, and whilst under the influence of terrestrial Magnetism alone. The ratio of the vibrations being as 12 : 1·5, whilst the iron acted in concert with the force of terrestrial Magnetism ; and the latter alone producing one vibration in the same interval of time, the ratio of the forces of iron, under the influence of the bar-magnets and of the earth, is as $(12^2—1^2) : (1{\cdot}5^2—1^2)$, or 114 : 1 nearly.

99. This ratio ascertained, the system of magnets and the counteracting magnet were replaced, so as to give permission to the needle to swing as if under the force of terrestrial Magnetism only. The needle was now drawn 90° from the magnetic meridian, by means of a small magnet, and permitted to rest in that position whilst the iron was heating.

100. When the iron had attained a bright red heat, approaching to whiteness, it was placed in its former position with respect to the system of magnets and the compass needle, having one end resting on the magnets and the other on the edge of the brick. The needle was carefully watched, but not a movement of it was observed whilst the iron was at a high red heat. As the iron cooled, the needle began to yield to its commencing magnetic action: at first its movement was very slow, but in a short time it advanced rapidly towards the meridian, and finally settled in that plane. In this experiment, as in those in which terrestrial magnetic influence alone was employed, the iron evinced a decided neutrality whilst at a high temperature; and the general features of the phenomena, under both circumstances, were precisely alike.

101. This experiment was repeated several times with the same results ; and although it might appear to be sufficiently demonstrative of the entire absence of magnetic action in highly-heated iron, yet as I thought that I perceived the magnetic action appear at a higher degree of temperature in these experiments than in those in which the terrestrial magnetic force alone was employed, it seemed prudent to augment the

polarizing force still further. I therefore nearly doubled the polarizing force last used in some subsequent experiments, but in no case could magnetic action be detected when the iron assumed a high red heat.

102. I next subjected cylindrical bars of wrought iron, highly heated, to the action of powerful electric currents from a Voltaic battery, consisting of eight pairs of cast iron jars and amalgamated zinc plates, arranged in a series of four double pairs, and excited by a strong solution of sulphuric acid. The iron bars were placed in a brass tube, inclosed in a spiral conductor, from which it was insulated by intervening stout brown paper; but although the iron bars, when cold, would display a powerful attractive force, by virtue of the electro-magnetic action of similar currents, not a trace of magnetic action on a delicately-suspended magnetic needle, beyond that due to the spiral, could be discovered when the iron was at a high red heat. But in these experiments, as in the former, the iron gradually resumed its magnetic functions as it cooled down towards blackness.

103. The results of the several series of experiments hitherto enumerated appear to me to be conclusive evidence regarding the entire absence of magnetic powers in ferruginous bodies, when highly charged with calorific matter; and I cannot avoid regarding them in the important capacity of so many experimental demonstrations of the calorific matter being capable of *expelling* the whole of the magnetic matter from ferruginous bodies, and consequently as unexceptionable evidence of the perfect distinction of their physical characters and attributes.

104. Whilst these investigations were going on, others, no less important, presented themselves to my mind, and the experimental results happened to correspond with the views I had taken. They are perfectly novel, and, I believe, were never before thought of by any other experimenter.

105. The facts on which I reasoned were these:—1. That a bar of iron, as I have shown (80-83), can be formed into several distinct magnets, by the introduction of calorific matter to intervening portions of the iron—the whole bar being under the feeble polarizing force of terrestrial Magnetism. 2. That, as has long been known, every individual fragment of a broken magnetic bar of steel becomes a perfect magnet—it appeared to me very likely that, instead of employing mechanical violence to break a bar magnet into several smaller ones, the same end might be accomplished by the agency of heat; and that a magnetized steel bar, heated in various places, might be converted into as many distinct magnets as there were parts of it left in a comparatively cool condition.

106. My first experiment in this new inquiry was made on a bar-magnet, about eight inches long, one inch broad, and a quarter of an inch thick. I retouched this bar previously to heating it, and ascertained the position of its poles, which were found to be nearly a quarter of an inch from its ends.

107. One end of this bar was now placed in the fire, so as to render more than one-third of its length red hot. When the heat seemed to be high enough, the bar was withdrawn from the fire, and placed with its cold part on a brick. In this condition a compass needle was presented to various parts of it, and indicated that the heated end was perfectly neutral, and that the cold part was a perfect magnet, having one of its poles at the extreme cold end of the bar, and the other pole distinctly marked where the metal began to be red hot.

108. As the metal cooled the latter pole advanced on the heated part, and eventually assumed a diffused character, similar to that shown by that end of soft iron which is attached to the pole of any bar-magnet.

109. My next experiment was on a bar-magnet, about sixteen inches and a half long, three-quarters of an inch broad, and half-an-inch thick. This bar was heated in the middle part, whilst the two extremities, each about one-third of the whole length, were kept as cool as possible by frequent applications of cold water. When the central part had arrived at a bright redness, the bar was taken from the fire and placed on two bricks, one under each end. Thus situated, it was examined by a compass needle, and found to be converted into two distinct magnets, one at each end, or cool part, and separated from each other by the central heated part, which was perfectly neutral.

110. These two experiments were the only ones made in this inquiry; for as they had produced the results I had expected, and thus verified the theoretical views I had entertained, it was easy to predict that any bar magnet of sufficient length might be made to display as many distinct and perfect magnets as we please, by the influence of heat alone, when applied in a similar manner.

111. The difference in the mechanical and the calorific modes of subdividing a magnet into several smaller ones consists in the sub-division of the metal in the former case and the sub-division of the magnet only in the latter. By the former process, however, the new poles are permanently fixed in the steel; whereas, by the latter, they are transiently displayed, and change their situation as the metal cools. Eventually the heated parts, after cooling, assume a diffused polarity.

W. S.

Manchester, *February*, 1843.

AN EXPERIMENTAL INVESTIGATION OF THE MAGNETIC CHARACTERS OF SIMPLE METALS, METALLIC ALLOYS, AND METALLIC SALTS.

TWENTY-SIXTH MEMOIR.*

PART I.

(Read before the Manchester Literary and Philosophical Society, April 7th, 1846.)

1. The subject to which this Memoir is devoted is one that has been already explored, at different periods, by many eminent experimental philosophers who have recorded the results of their labours in various works on science; and as several interesting and well-attested facts have been discovered by those investigations, but little would appear to be wanting at this time to accomplish all the desirable information respecting the magnetic action of bodies, whether of a ferruginous or a non-ferruginous character. Under these circumstances any further investigation of mine, or of any other experimental inquirer, could be productive of nothing more than the development of a few novel facts in addition to those already recorded, or the application of some of them to novel and useful purposes.

2. It frequently happens, however, in experimental inquiries of this kind, that the different modes of investigation resorted to by different philosophers are not only productive of new facts, but are the means of developing new laws, and of leading to theoretical views, differing considerably from those previously entertained for the explanation of phenomena, which had long been grafted into the history of science. And it must be acknowledged that, whether new facts be added to the old stock, the application of any of them to useful purposes discovered, or that novel and more exact views for the explanation of those facts be developed, an additional step in the advancement of science would thus be securely established; and it is solely from a hope that, by *some* of these means, the present Memoir will contribute to the progress of scientific knowledge that I have ventured to offer it to the consideration of this Society.

3. The superlative degree of magnetic action displayed by metallic iron above that of all other known bodies, has been a theme of great import, commanding much attention and philosophical speculation from remote periods in the history of science till the present day, and continues a subject of high interest and admiration throughout

* Manchester Memoirs.

every part of the scientific world. The ordinary laws of the magnetic action of metallic iron, especially when in masses, are, however, now so satisfactorily established, and the phenomena so well known, that any further notice of them in this place would be foreign to the object of this Memoir—more especially as it alludes to the simple magnetic attractions only, whilst illustrating the novel facts it contains, and the mode by which they were developed.

4. Nickel is a metallic body which, next to iron, stands most distinguished for the display of magnetic action; and, indeed, notwithstanding the number of inquiries that have been made respecting the magnetic action of other bodies, and the talent and expedients that have been employed in the pursuit, little or nothing has been satisfactorily ascertained beyond that which is so conspicuously displayed by those two metals, iron and nickel (7-13).

5. There is something very remarkable, however, respecting the magnetic action of those two distinguished metals when in combination with other bodies. Nickel, for instance, is said to lose all its magnetic action when combined with even a small dose of arsenic, and iron has long been understood to suffer the same fate when alloyed with antimony. Beyond these two alloys of nickel and iron, I am not aware that any have been magnetically investigated—although, as will appear in the sequel, some of the most extraordinary facts that have hitherto appeared in the Magnetism of metallic bodies are displayed by alloys of iron and nickel with other metals.*

6. Tiberius Cavallo was amongst the earliest inquirers into the magnetic action of non-ferruginous metals; but the principal part of his experiments were limited to copper and brass—specimens of both of which he found to be magnetic, and especially after they had suffered the operation of hammering. The investigating apparatus of this philosopher, like that of many subsequent inquirers, consisted of a delicate magnetic needle, to the poles of which the specimens under examination, to prevent commotion in the air, were slowly and dexterously presented.†

7. As brass is an alloy so extensively employed in the construction of magnetic compass boxes, its magnetic or non-magnetic condition is an important scientific inquiry, which, though for many years in the hands of philosophers, remains at this day as undetermined as when first undertaken. That certain pieces of brass have displayed unequivocal magnetic action is a fact which cannot be questioned, but whether that action was due to the alloy of pure copper and zinc alone, or to portions of iron accidentally present in the metal, different opinions have been entertained.

* At the time this part of the Memoir was read, I was not aware of these curious and interesting facts. They were subsequently discovered, and are described in Part II.

† Cavallo's experiments were both extensive and interesting. They led him to infer that the Magnetism displayed by hammered brass is not due to any iron that it contains, "but to some peculiar configuration of its parts."

8. It has been supposed by Cavallo and other philosophers, that all bodies, whether metallic or otherwise, are endowed with magnetic powers which vary considerably in degrees of energy whilst under the influence of, or operating on, the magnetic needle. But when we meet with such conflicting opinions as those that appear in the writings of philosophers so eminent in this department of physics as Cavallo, Coulomb, Bennet, Halley, Biot, Becquerel, and others who have entered the field of research, we are necessarily led to infer that the subject has not yet been accurately and satisfactorily determined.

9. The beautiful experiments of Arago* and others, that preceded the development of Magnetic Electricity by Dr. Faraday, afford an ample explanation of nearly all the results of those experiments in which vibrations of the magnetic needle, near the bodies under examination, were taken as evidence of their magnetic actions, as well as in all those cases in which light needles or morsels of the bodies examined were vibrated under the influence of powerful magnets. It is reasonable to suppose also that thermo-electric currents would influence the results of those experiments which were made previous to the discovery of that branch of Electricity by Dr. Seebeck, especially in those cases in which the bodies under examination were held in the hand whilst presented to the magnetic needle.

10. There are, however, some phenomena on record, the explanation of which do not appear to fall within the range of the laws of either Magnetic Electricity or Thermo-Electricity; and, therefore, the cause of their development is necessarily located in some other source. For instance, when Coulomb employed light, delicately-suspended needles of gold, silver, glass, wood, and other substances, both organic and inorganic, he found them to obey the polar forces of the magnet in precisely the same manner as needles of iron would do; for after the vibrations had ceased, those needles became arranged between the poles of powerful magnets in such a manner that the axis of each needle rested in the line of magnetic force, or in a right line joining the south and north poles employed.

11. It is somewhat remarkable that, when similar experiments were made by M. Becquerel, the results were very different. By employing needles of wood, lac, and some other substances, this excellent philosopher found that the positions they assumed, when at rest directly between the north and south poles of powerful magnets, were invariably at right angles to a right line joining those poles; and consequently at right angles relative to the magnetic force, or to the position in which the needles of Coulomb rested. From the results afforded by the experiments of M. Becquerel, that philosopher has been led to consider that the effects produced by a strong magnet on a magnetic needle, or on soft iron, differ essentially from those which take place in all bodies whose original Magnetism is very weak. In the former, the mag-

* See Ninth Memoir, page 216.

netic axis of each is arranged in its *length*, but in the latter class of bodies the magnetic axis becomes arranged transversely. M. Becquerel shows, however, that wooden needles assume different positions with respect to the magnetic poles, according to the distance at which they are placed from them.*

12. In a paper by Dr. Faraday, read before the Royal Society of London, in January last, it is stated that a variety of bodies—bismuth being the most eminent in this respect—arrange themselves, with regard to powerful magnetic poles, in precisely the same manner as described by Becquerel—that s, with their longest axes at right angles to the line of magnetic force.

13. Dr. Faraday has attempted a classification of a great number of bodies under the two following heads—" Magnetics" and " Diamagnetics." The former class, of which iron is the grand type, become arranged, whilst under magnetic influence, with their longest axes in the magnetic line of force; and the latter class, of which bismuth is the type, become arranged at right angles to the magnetic line of force. In the magnetic class, Dr. Faraday places some of those bodies which, according to M. Becquerel's nomenclature, would be placed in the other class, or amongst those which become arranged at right angles to the line of magnetic force. Such, however, is the condition of this interesting inquiry at the present time, no two of those hitherto engaged in it having arrived at similar results in any series of experiments that have been undertaken.

14. The inquiries that I have made in this department of magnetics have been conducted partly by the employment of magnetic needles, partly by permanent steel magnets, and partly by electro-magnets, which have afforded different modes of assailing those substances that were the objects of investigation.

15. In all cases where delicate magnetic needles are employed, especially when the suspension is by means of a fibre and the system astatic, the experiments are exceedingly tedious, and much time is required to allow of the system's repose from its agitations before an attempt can be made to approach it with the specimen to be examined. When, however, a single needle is employed, whose support is a finely-pointed pivot, the experiments are less subject to delay than by the other mode, though much caution and some dexterity are still required to enable the experimenter to arrive at satisfactory results; but in whichever way the magnetic needle may be suspended in these delicate investigations, the bodies under examination must either be held immediately in the hand, or indirectly by means of some other body previously ascertained to have no influence on the needle. If held in the hand, thermo-electric currents are to be suspected; and if attached to the end of a wooden rod, by means of sealing wax or resinous cement, other electric actions may interfere with the results, or may, indeed, be the sole cause of any motions that may happen to be observed.

* Traité Experimental de l' Electricité et du Magnetisme, tome ii. page 387.

Moreover, a delicate magnetic needle does not possess a sufficient degree of power to bring into play the minute portions of Magnetism that lie dormant in many bodies. These exiguous sleeping forces can never be roused into a state of activity, and consequently can never be discovered, by merely presenting the bodies in which they reside to the pole of a feeble magnetic needle. To accomplish their discovery, a comparatively powerful magnetic action is absolutely required; for, when thus assailed, their polarization is more easily enforced, and their detection almost certain. The magnetic needle, however, may be usefully employed in cases where the suspected Magnetism of a body is of some easily detected amount; and it may be resorted to with advantage in preliminary trials, under all circumstances, because of the possibility of the specimen under examination possessing a sufficient amount of Magnetism to be detected by it, and the more tedious modes of inquiry being thus rendered unnecessary.

16. I have found that a convenient and efficacious mode of examining bodies, the magnetic actions of which are very feeble, and others in which Magnetism has but a questionable existence, is by means of an apparatus represented by Figs. 13 and 14, Plate XVII. Fig. 13 is that part of the apparatus in which the specimens to be examined are placed. It consists of a light cylindrical wooden rod, A B, about twelve inches long, and suspended by a few parallel fibres of silk, F F, from the cocoon. The end, B, is furnished with a light slip of card paper, and two loops of horse-hair, for the purpose of holding the specimen—say a half-crown, for instance—as represented in the figure, which is counterbalanced at the other end of the lever by a sliding weight. This part of the apparatus is inclosed in a rectangular box, Fig. 14, whose ends, top, and one of its sides are of glass; and a brass tube rises from the middle of the top, in which hangs the silken fibres. The head of this tube sustains the fibres and their appendages, and can be turned in any horizontal direction, for the adjustment of the lever to a parallelism with the sides of the box.

The glass parts of the box are sustained by a light mahogany frame, with a bottom of the same kind of wood. The ends and side are fixed, but the top, which consists of two sliding parts, can be removed at pleasure, for the purpose of introducing the hands for the adjustment of the apparatus within, and replaced when the specimen has been accurately counterpoised—a process which is still further facilitated by the introduction of a hand at one side of the box, which is opened for that purpose, and afterwards closed by a sliding mahogany door.

When the agitations of the lever have subsided, the sliding door is partially opened for the introduction of the poles of a powerful horse-shoe magnet, which is made of a long and narrow shape for that purpose. This magnet is placed on a sliding carriage, by means of which it is made to approach the specimen or recede from it with great facility and in the most gentle manner. In consequence of finding decided polarity

in some specimens in which no Magnetism was previously known to exist, I have been led to the employment of a powerful bar-magnet, which I find convenient in those cases where such polarity is suspected. I have also employed electro-magnets, both straight and of the horse-shoe form ; but having found that these are troublesome and inconvenient, I have abandoned the use of them altogether in these inquiries.

In cases where extreme nicety is required, I have found the following mode exceedingly useful. Besides the sliding door, which closes one side of the box, I have another sliding piece, P, which fits into the same grooves in the side of the box. Through this piece pass two cylindrical rods of soft iron, *i i*, about two inches in length. They are firmly fixed in the wooden slider at their middle parts, and parallel to each other. When this piece is in its place, the iron rods are in one and the same horizontal plane, having one half within and the other half outside the box, and their inner ends presented to the specimen suspended on the lever. When the remaining portion of that side of the box is closed by the sliding door, the specimen is nicely adjusted to the ends of the iron rods, by turning the top-piece of the tube until the most trifling space is perceptible between them. When all is at rest, the poles of the horse-shoe magnet are made to approach the outer ends of the iron rods, bring them into magnetic action, and thus detect the Magnetism of the specimen, if any exist in its structure.

17. By the assistance of this apparatus, to which I give the name "*Torsion Magnetoscope*," I have examined gold, silver, copper, platinum, tin, antimony, lead, zinc, bismuth, and mercury, and also some of their alloys and salts. In none of these metals, when in a state of purity, have I been able to discover the slightest trace of magnetic action, though in several specimens of some of them, as they appear in a commercial state, considerable magnetic action is speedily developed.

18. A bar of bismuth, for instance, cast from a mass fused in an earthenware crucible, was found to be highly magnetic, and, for a while, was considered as a good specimen of the magnetic action of that metal ; but, on examining another bar, cast from the remaining portion in the crucible, and finding it still more powerfully magnetic than the former, a suspicion was aroused that either their crystalline structures were different to each other, or that the metal was not pure. The experimental inquiries which this suspicion occasioned led to the detection of localities in the two bars in which the magnetic actions were more powerful than in other parts of them, which gave rise to the determination of *sweating* one of the bars at a low heat, and running out of the crucible the most easily fused portions before the rest became fluid, which is an excellent process for freeing bismuth from some of the impurities with which it is frequently contaminated in the mercantile state.

19. This purified bismuth, having been cast into a bar, was afterwards broken into convenient fragments and tested by the torsion magnetoscope, previously described

(16), but not the slightest trace of Magnetism could be detected in any of the pieces.

20. Having satisfied myself that no magnetic action resided in the pure bismuth, the dross left in the crucible was softened by heat and poured on a stone slab; and, on being tested, displayed high magnetic powers. It now became obvious that the whole of the Magnetism detected in the bismuth, when in its first state (17), was due to that portion only which was left as dross in the crucible after the pure metal had been run out.

21. From the results thus arrived at, I was induced to fuse other masses of mercantile bismuth, and run out the purest portions of the metal at the lowest degree of fusible heat, by which means I have been enabled to separate the magnetic from the unmagnetic parts, and thus to arrive at the conclusion that pure bismuth is not susceptible of any *direct* magnetic action by the mere approach of the poles of a powerful magnet—which I consider a test of far greater certainty and exactness than that of a feeble magnetic needle, whatever may be the delicacy of its suspension; and as a peculiar class of phenomena become displayed by a sudden development of the powers of an electro-magnet, much misconception might arise from its employment.

22. The most usual impurities of bismuth of commerce are sulphur and arsenic, and occasionally a small portion of silver and iron. On subjecting the drossy part (19) to dilute sulphuric acid, a portion was dissolved, after which the liquor was reduced almost to dryness by evaporation. The residue being much diluted with pure water, and a solution of ferrocyanuret of potassium being added, it assumed a strong blue colour, which indicated that a portion of iron had been dissolved from the mass. On dissolving another portion of the dross (19) in dilute sulphuric acid, the presence of iron was again indicated by the addition of bruised gall-nut. Similar tests were applied to portions of the pure bismuth, but no indication of iron was observed. Hence it was fair to infer that the whole of the magnetic action displayed by the commercial bismuth (18), first tried, was due to the iron it contained.

23. I have examined antimony in the same way, and have found some specimens magnetic, and many others in which magnetic action could not be detected. By thus operating on antimony, however, it would be impossible to form a correct idea of the magnetic or unmagnetic state of that metal; because, as will appear in the sequel, of its masking, to a considerable extent, the Magnetism of even considerable portions of iron, when the two metals form a perfect alloy. Hence, in order to test antimony magnetically, it becomes necessary to ascertain, by chemical processes, that it is perfectly pure, and especially that it is free from iron; for although antimony will mask the Magnetism of iron, when in perfect unison with that metal, a very trifling portion of uncombined iron will render the whole mass *apparently* magnetic. By attending to these particulars, pure antimony will not be found to display any magnetic action (17).

24. It is generally understood—principally, I believe, upon the authority of Dr. Seebeck, of Berlin—that iron becomes " completely destitute of magnetic action," when alloyed with four times its weight of antimony.* This, however, does not appear to be correct, for I have formed very perfect alloys of these two metals, in a great variety of proportions, and find that when the iron does not form a twentieth part of the mass it is still magnetic, though in a very low degree. When the alloy is of equal parts of iron and antimony, it is highly magnetic. This alloy, when broken, exhibits a dark grey fracture, somewhat glittering. It is easily reduced into a powder by the operation of the file, or by pounding in a mortar; and, what is very remarkable, it yields an abundance of deep crimson sparks when struck against hard steel.

25. It has already been stated (17), that pure copper is not magnetic ; and I must now add, that in a very few cases only have I detected magnetic action in the copper of commerce, although I have tested a great number of specimens both in the state of sheet and of wire.

26. In the copper coinage of this country I have never yet met with any magnetic action, notwithstanding the number of experiments I have made on the various copper coins that have been struck in the reigns of several sovereigns.

27. In the gold and silver coinage, however, in which copper forms a constituent part, the case is very different. These alloys are nearly all of them decidedly magnetic, and probably none of them entirely free from Magnetism. The gold coinage, however, displays much feebler magnetic action than the silver coinage ; indeed in many gold coins the existence of Magnetism may be considered as questionable, whilst in others, and especially in those of 1844, magnetic action is prominently displayed.

28. The silver coinage, although in some specimens scarcely any magnetic action can be detected, is generally magnetic in a very eminent degree ; and I have found that, when any one piece of a particular coinage displays considerable magnetic action, the whole of that coinage, as far as I have examined it, is similarly magnetic. And, on the other hand, when a silver coin has been found to be but very slightly magnetic, I have but rarely met with one of that particular coinage in which any considerable degree of Magnetism could be detected.

29. Of the silver coins that have come under my notice, a half-crown of William and Mary, dated 1691, is the most eminently magnetic. The next, in point of magnetic action, are the half-crowns of 1844 and 1845 ; then one of George IV., the date of which I have not noticed. The half-crowns of George III., of 1819 and 1820, are more slightly magnetic than those last named ; and the half-crowns of both coinages in 1817 are still less magnetic than those of 1819 and 1820. Shillings also of certain coinages are magnetic in an eminent degree ; and there are but few, if any, that I have examined, that are entirely neutral to the high magnetic powers with which they have been assailed.

* Brewster's Magnetism, page 102.

30. Silver articles for domestic purposes, such as spoons, prongs, fruit knives, &c. were, in many specimens, found to be much more magnetic than any of the silver coins that I have examined. I have tested several sets of silver tea spoons, belonging to neighbouring families, and, with the exception of one half-dozen of Scotch spoons, of very old date, all of them have displayed high magnetic powers, though of very different degrees in different sets. But what is very remarkable, if one individual spoon was found to be highly magnetic, the whole of that particular set, whether it consisted of half-a-dozen or a dozen spoons, were highly magnetic also. And generally, whatever might be the magnetic condition of any individual spoon, the whole number of the set to which that spoon belonged were magnetic alike, or nearly so. Hence if the quantity of magnetic action of any individual spoon were to be denoted by q, and the number of spoons in the set denoted by n, the sum total of magnetic action in that set of spoons would be $n\,q$ nearly. Of course this reasoning applies only to individual sets of spoons which are of uniform make, composition, and structure of metal. It appears also, as far as my experience has extended, that the same mode of reasoning would give the sum total of all the magnetic action that any individual coinage would display. Suppose, for instance, the Magnetism displayed by a half-crown piece were to be taken as the unit of quantity equal to q, then the number of pieces being n, the sum total of Magnetism which the whole of that coinage would display would be $n\,q$ nearly, and similarly for any other individual coinage of silver.

31. The difference of magnetic action displayed in the silver coinage and domestic articles of that metal (29-30), led to the supposition that minute portions of iron might accidently have got introduced to the alloys, whilst in a state of fusion, which had some probability in its favour, from the fact that the metal for silver coinage is fused in cast iron pots,* and therefore liable to take up portions of those vessels; but on the other hand, if that were always the practice, it would lead to the inference that in all the silver coins the iron would be nearly in the same proportion, and the extent of magnetic action almost the same in all; whereas, by the tests already described, this is not the case.

32. The current silver coinage of William and Mary became so base that, in the year 1694, it was all called in and a new coinage issued. From this fact it occurred to me that there was a possibility, at least, that the high degree of magnetic action displayed by the half-crown of 1691 (29), was owing to an undue proportion of copper, or of some other inferior metal. This idea led to the selection of a shilling, in which scarcely a trace of Magnetism could be detected, for fusion, with an additional portion of copper, also non-magnetic, having been obtained by the electro-type process. These, together with a piece of pure silver, were fused in an earthenware crucible, and

* Ure's Dictionary of Arts and Manufactures, also Brand's Chemistry, 1037.

run out upon a sheet of copper. The copper in this alloy was about one to five of silver, which is more than twice the proportion of that in the standard coinage. On subjecting this mass to the torsion magnetoscope, it was found to be more highly magnetic than the old half-crown of William and Mary (29).

33. This singular result has cost me much thought, and a great deal of trouble. The crucible employed was quite clean, having never been used before ; and its contents, during the time it was in the fire, were the silver and copper, and a mixture of pulverized charcoal and common salt. Similar pieces of charcoal and slices of the same quality of salt have been tested, but no magnetic action could be detected in either. Whence then this almost unexpected Magnetism in the metallic alloy? Fragments of a broken crucible similar to that used were found to be slightly magnetic, probably from a portion of iron in the clay of which it was formed; but the magnetic action in the fragment of the crucible was not nearly so great as that displayed by the alloy. Moreover, the pure portion of the silver that entered the alloy had previously been fused in a similar crucible (one of the same nest), and with a similar mixture of pulverized charcoal and salt, and yet showed no trace whatever of magnetic action. Hence it could hardly be imagined that the Magnetism displayed by the alloy was due to iron derived from the crucible.

34. After subjecting a portion of the alloy (32) to diulte nitric acid, and finding no iron in the solution, the surface of the metal was washed in clean water and thrown into dilute sulphuric acid, which, upon the principles of Electro-Chemistry, took up a portion of the copper, and had iron been present, would have taken it up also. But no indication of that metal appeared by the test of gall nut, nor by that of ferrocyanuret of potassium ; but the ferrocyanuret of copper was manifested in an eminent degree, though previously to the introduction of the ferrocyanuret of potassium scarcely any colour was perceptible in the liquor.*

35. From the facts above described (32, 33, 34)—and at present I rest on no other data—I am inclined to think that, if any iron could possibly have entered the alloy, its quantity must have been too small to cause the high degree of magnetic action which the specimen exhibited.

36. On comparing this alloy of silver and copper with the alloy of iron and antimony, in which the weight of the latter metal is only about twenty times that of the iron (24), some very remarkable circumstances present themselves. In the former alloy, where no iron can be detected by the usual chemical tests, we have a metal whose magnetic action is, at least, twice as powerful as that displayed by the alloy of iron and antimony—an alloy of which iron constitutes a very considerable proportion, and whose presence, had it not been previously known, could have been detected by the humblest chemical test. These parallel experiments tend to show either that an

* Another portion of the alloy has since been analyzed with similar results.

alloy of pure silver and pure copper is magnetic, or that the magnet is a better test for the presence of small portions of iron in such alloys than any hitherto known in chemical manipulations.

37. I regret that another piece of unmagnetic silver has not yet fallen in my way, to enable me to make further investigations on this curious subject; but I am in hopes of obtaining unequivocal results before the second part of this Memoir is brought before the Society.

38. With respect to brass, one of our most important alloys, I have found it to be highly magnetic in a great number of cases, viz.:—In all the various states of *newly-cast* brass, brass wire, and sheet brass, as well as in several articles of brass manufacture. But, from the great difference in the *degrees* of magnetic action displayed by different specimens of this beautiful alloy, and the total absence of that action in others, there has appeared to me a high degree of probability that the Magnetism displayed by brass is due to accidental portions of iron in the alloy.

39. Cavallo discovered that in magnetic brass the action was more powerful in large pieces than in small ones. This fact I have also observed in several specimens that I have examined. The reason seems to be, that in the larger pieces a greater quantity of ferruginous matter (if present) is brought into operation than in the small ones.

40. Cavallo also states that hammering an unmagnetic piece of brass will cause it to become magnetic. An instance of this kind I have never yet met with; but I have found that, when an unhammered piece has been so slightly magnetic as to have that character but just discernable, hammering it, so as to compress its two sides closer together, gives it an increased magnetic action, which may possibly be a consequence of bringing the whole of its magnetic particles more completely within the range of the testing influence; and I am inclined to believe that, had Cavallo's test been more powerful than a magnetic needle, he would have found that those pieces whose Magnetism he thought was due to compression alone were slightly magnetic previously. Still, however, there is a possibility that Magnetism might be detected in compressed brass, in which that power was too feeble to be detected whilst the metal is in an uncompressed state, even by powerful magnetic tests.

41. In addition to the advice given by Cavallo, respecting the necessary caution in employing brass in the construction of compass-boxes, I should advise the makers of those useful instruments to test every piece of brass intended to be employed in their construction by a powerful magnet, instead of testing them in the usual way by means of a delicate magnetic needle; and, if this test be accurately performed, when the metal first arrives from the foundry, the detection of any concealed Magnetism, even if very feeble, will be almost certain; and much of that labour and uncertainty, which must always attend examinations by the needle, would be avoided. Unfortunately, however, too much reliance is usually placed on the appearance of the brass, or on

the character of the foundry whence it is procured, and the consequence is that but very few brass compass boxes, that are in common use, are entirely free from magnetic action.

42. The next alloy of importance that I have examined is German silver, in which nickel is one of the principal constituents. In the best kind of German silver (constituted of copper eight parts, nickel six, and zinc three) a slight magnetic action has been detected, but in the inferior kinds of German silver, into which only about three parts of nickel enter, I have not detected any magnetic action whatever. Hence the Magnetism of that portion of nickel is obviously neutralized in that particular alloy.

43. The metallic salts that I have examined are some of those most frequent in common use. The salts of iron were the sulphate, the yellow, and the red ferrocyanuret of potassium, also Prussian blue. These, with the exception of the yellow prussiate, are magnetic—the sulphate of iron in the highest degree of any of them. It is somewhat remarkable that the two kinds of prussiate of potash, where the proportions of iron are nearly alike (yellow 15 per cent. red 16 per cent.) should display such a material difference in their magnetic characters; and it is still more singular that Prussian blue, which contains more than 45 per cent. of iron, is less magnetic than the red prussiate of potash. The sulphate contains about 33 per cent. of iron, and is the most magnetic of the whole.

44. Pure sulphate of copper shows no magnetic action, but much of that of commerce is highly magnetic, being, as I have ascertained, adulterated with sulphate of iron. Hence the magnet would be a good and speedy test for ascertaining the quality of the commercial salt.

45. The following salts appear to be perfectly neutral to magnetic forces:—Common salt, saltpetre, borax, sulphate of magnesia, sulphate of soda, sulphate of potash, and carbonate of soda.

46. Thus far I have attempted to contribute to the list of facts previously known, and to correct some errors which, probably on account of the insufficient modes of investigation, have crept into this particular branch of science. It is possible also, I think, that some of the facts which have now been pointed out may be an inducement to employ the magnet more extensively than hitherto in the laboratory of the chemist.

AN EXPERIMENTAL INVESTIGATION OF THE MAGNETIC CHARACTERS OF SIMPLE METALS, METALLIC ALLOYS, AND METALLIC SALTS.

TWENTY-SIXTH MEMOIR.

PART II.

(Read before the Manchester Literary and Philosophical Society, May 5th, 1846.)

47. In addition to the facts enumerated in the first part of this Memoir, further investigations have led to the discovery of others of a no less interesting character. By extending the examination of British silver coins, I find that the whole of them are more or less magnetic. The half-crowns of the present reign that have come under my observation are certainly all magnetic, and some of them display considerably high magnetic powers, more especially those of the years 1842, 1844, and 1845; and a Victoria shilling, coined in the year 1842, is still more magnetic than any of the half-crowns.

48. The half-crowns of George the Fourth are in general highly magnetic, though but very few of them display such high magnetic powers as some of those of William the Fourth. There is something remarkable in the following fact:—I have not met with any of the half-crowns of George the Third that are so powerfully magnetic as those of subsequent coinage.

49. With respect to silver ornaments, silver medallions, and silver articles for domestic purposes, they differ materially in their magnetic conditions; but generally they are more highly magnetic than the British silver coins (30).

50. The large silver medal of the Society of Arts, for 1825, weighs nearly two and a half ounces, yet it does not display even the slightest degree of magnetic action, by the most severe test to which it has been subjected; whilst a small silver medallion of the Commonwealth, representing Lord Essex on one side and the two Houses of Parliament on the other, was found to be more highly magnetic than any of the previously-named coins. A small silver medallion of Charles the Second exhibited a slight degree of magnetic action.* An Indian rupee that I examined showed no magnetic action whatever.

51. The present gold coinage of this country is in general but feebly magnetic. I have, however, met with a few sovereigns of the present reign which are more magnetic than any others that have come under my notice.

* For the use of these medallions and the Indian coin, I am indebted to my friend G. Wareing Ormerod, Esq.

52. With respect to jewellery, it is generally more highly magnetic than any of those silver articles that have been examined (30). Wedding rings, which contain but a small proportion of copper, possess so slight a degree of magnetic action as almost to elude the detection of it; whilst ornamental rings, keepers, &c. which contain a much greater proportion of copper, are generally highly magnetic. Some ear-rings that I have examined are still more highly magnetic than the finger-rings. Gold watch-chains are generally magnetic, especially those containing much copper; also gold spectacle frames, unless they be of what is called fine gold, are magnetic to a considerable extent. In all cases where steel or iron screws or nails have been found in the gold articles examined, those parts have been carefully removed previously to the magnetic test being applied.

53. We next come to the consideration of metallic alloys, of which either iron or nickel form no inconsiderable proportions. It has already been shown in the first part of this Memoir (24), that antimony, when alloyed with iron, counteracts the magnetic action of the latter metal in a very eminent degree, rendering it almost undetectable when the iron amounts to little less than a twentieth part of the mass; and I find that, when the ferruginous metal amounts to no more than about a fortieth part of the mass, its magnetic powers entirely disappear.

54. There are several other metals, besides a number of other bodies, which either partially or wholly, neutralize the magnetic powers of iron and nickel. The most eminent of the metals in this capacity is zinc. This metal, which, till these researches were undertaken, was not known to affect the Magnetism of iron, neutralizes nearly the whole of that power when alloyed with an equal proportion of the ferruginous metal; and although, from an accident with the melting pot, I have not yet arrived at the fact, I have no doubt whatever that, when the iron amounts to no more than one quarter of the mass, its alloy with zinc will be perfectly neutral to the magnet.*

55. The neutralization of Magnetism in iron, by alloying it with zinc, is a fact of high importance in the contemplation of metallic Magnetism, and especially the Magnetism of brass and other alloys in which zinc forms a considerable proportion; for it is highly probable that, since zinc smothers the magnetic influence of large proportions of iron, a considerable quantity of the latter metal might enter the composition of brass without rendering it palpably magnetic. Such, in fact, would absolutely be the case, provided the alloy were perfect, and that the copper had no influence on the magnetic condition of the combined iron and zinc.

56. To satisfy myself on this point, I have subjected to chemical analysis some of those specimens of brass which had been found to be highly magnetic; and, as far as

* Since this Memoir was read, I have ascertained that an alloy of iron and zinc, in the proportion of 1 to 7 respectively, is quite destitute of magnetic action.

I have proceeded, there appears no reason to suppose that the magnetic powers they displayed were due to iron in their composition—an inference which, though very different to the opinion I had previously entertained (38), has been enforced by the facts that this part of the inquiry has developed. Indeed, I am now inclined to embrace the opinion of Cavallo, that the Magnetism of brass is not due to the presence of ferruginous matter, but depends upon a suitable arrangement of the particles of its proper constituent metals, copper and zinc. Nor do I believe that brass generally, as it leaves the foundry, contains any notable quantity of iron. I have analyzed many specimens, both magnetic and unmagnetic, and the traces of iron, where any were discoverable, were very minute and as frequent in the one kind as the other. It is true that some specimens of brass contain more than an average proportion of iron; but it is a curious fact that these specimens are not those which display the greatest magnetic powers.

57. We learn also, from these facts, that the demagnetizing powers of antimony and zinc will necessarily prevent the detection of small portions of iron residing in those metals, even by the aid of powerful magnetic forces, and leave us in uncertainty regarding their purity when examined by this test alone.

58. Lead and iron do not easily unite into a perfect alloy, excepting when the ferruginous metal is in very small proportions; but, when thus combined, the iron loses a great part of its natural magnetic qualities.

59. Silver and copper unite very sparingly with iron; but whether the magnetic powers of the latter are affected by its union with those metals or not is not yet known.

60. In order to ascertain the exact quantity of pure metallic iron that would make a neutral half-crown* *apparently* magnetic to the same extent as another half-crown was *absolutely* magnetic, I attached to the former, by means of softened gum, new iron filings; and, after many trials, ascertained that the requisite quantity of filings amounted to about a ten-thousandth part of the mass; and on changing the pure iron filings for the peroxide of iron, a four-hundredth and eightieth part of the mass was required to render it equally magnetic with the *standard* half-crown. Now, as more than two-thirds of this oxide is iron, it follows that in this state the iron loses a considerable portion of its magnetic powers; and that the proportion of iron required in this case, to produce the standard degree of Magnetism in the mass, was little short of a seven-hundredth part.

61. Now, if any iron existed in the standard half-crown, uncombined with the other metals, it would probably have been in a state of peroxide;† and that more than an

* This piece of coin was not entirely devoid of magnetic action, but it approached nearer to a state of neutrality than any other I then had. Its action was very feeble indeed.

† It is possible that the iron, if any, might be in the state of a carburet; but even in that condition much of its magnetic powers would be neutralized.

eight-hundredth part of the half-crown must have consisted of iron, if its Magnetism was due to the presence of that metal.

62. Again, the magnetic action of this half-crown was considerably more feeble than that of the alloy, which has been chemically examined, and in which, if iron was present at all, that metal was in a less proportion than a twenty-thousandth part of the mass—which proportion, in a state of peroxide, and divided, as it necessarily must have been, through the whole alloy, would scarcely yield the slightest perceptible magnetic action. Moreover, if iron to that amount were even pure or uncombined, its quantity was far too small for the display of those high magnetic powers of which it was obviously possessed; and as there is a probability, at least, that the magnetic powers of iron become deteriorated by an alloy of that metal with silver or copper, or both, there is not the slightest reason for supposing that the Magnetism of the alloy in question (32) was due to any iron that it could possibly contain. Nor do I believe that the magnetic actions displayed by the coinage are traceable to the presence of iron.

63. It has already been stated (42) that the magnetic action of nickel is considerably neutralized, when combined with zinc and copper, in the alloy constituting German silver. Since that part of this Memoir was read before this Society, I have had an opportunity of alloying nickel with zinc alone, and have ascertained that, when the zinc is about eight or ten times the quantity of nickel, the alloy is perfectly neutral to the magnet. This alloy has a zinc-coloured fracture, and partially crystallized in the manner of zinc; but it is extremely brittle, and easily pulverized in a mortar.

64. Nickel and antimony combine with facility, and in an extraordinary manner. If two pieces of the metals, one of each, be placed side by side in a crucible, so as to touch one another, especially at their upper ends, the moment the antimony assumes a dull red heat (even a lower heat than that which commences its fusion when alone) the nickel bursts out into a fine scarlet glow, fuses and spreads over the antimony in a beautiful fluid state, and insinuates itself into the pores of that metal, rendering the whole mass soft, like paste or butter. If, whilst in this state, the crucible be removed from the fire, and permitted to cool gradually, the fracture of the button of this alloy, when broken, is of a much lighter colour than that of antimony. It is of a light grey, and very imperfectly crystallized. It is not so brittle as antimony, though still pulverizable in a mortar. When one-fourth of the mass is nickel, the fracture is very compact, and not unlike that of fine steel, but of a lighter colour. With these proportions, the alloy is somewhat malleable, and can be cut by a cold chisel.

65. From a retrospection of the facts developed by these researches, in connection with those previously known, we are led to observe a material difference in the magnetic character of bodies, when in their simplest or natural conditions; and that these natural magnetic characters become considerably modified when the simple or elementary bodies are variously combined—some simple magnetic bodies losing their natural

magnetic properties, and others displaying a new magnetic action of which, previously to combination, they appeared to be destitute. Under these circumstances, it would be difficult to ascertain the line of demarcation between those bodies that are naturally and separately magnetic and those that are not. Probably the safest way would be to allow all bodies to possess more or less of the magnetic character; and to classify them into those that are *palpably* magnetic, like iron and nickel, and those that are but obscurely magnetic, or whose Magnetism is not detectable in their individual insolated states, but which become magnetic by combination.

66. Provisionally, therefore, we might venture to call the former class *Sapha** *magnetics*, and the latter class *Asapha*† *magnetics*.

67. *Sapha-magnetics* might be conveniently sub-divided into *Mono*‡ *magnetics* and *Suno*§ *magnetics*, accordingly as they consist of individual or of compound bodies. Then, as we have many bodies which counteract the highest magnetic powers of simple bodies, these might be called *Kata*|| *magnetics*, because many of them, if not all, have the power of completely neutralizing the magnetic actions of other bodies.

68. The *Mono-magnetics* at present known are but few in number—iron being the grand type. Next to iron is nickel. Cobalt is also a mono-magnetic body, and at present completes the list of this class of magnetics.

69. In the *Suno-magnetic* class I place alloys of copper and silver, copper and gold, and copper and zinc; and, although these three are the only ones with which we are yet acquainted, I have no doubt that many more alloys will soon find a place among suno-magnetics.

70. The *Kata-magnetics* are very numerous, as this class includes all bodies which, by combination, impair the Magnetism of other bodies. Amongst the metallic *Kata-magnetics* zinc is the most powerful hitherto ascertained. Next to zinc is antimony, then lead and tin. Arsenic, probably, stands very high in this class; but I have had no opportunity of ascertaining its proper place. The non-metallic *Kata-magnetics* are sulphur, oxygen, cyanogen, chlorine, carbon, and the generality of those bodies which combine with metals.

71. In proposing this classification of magnetics, I have aimed at nothing further than an abstract of that which absolutely takes place in nature. The whole rests upon facts, most of which have their analogies in Electricity. Therefore, this classification may be considered as supplying a small portion of an extensive nomenclature that has long been wanting in this region of science.

Manchester, 1836. W. S.

* Σαφῶς Clearly, manifestly.

† Ασαφῶς Indistinctly, without clear evidence or marks.

‡ Μόνος Alone, single.

§ Σύν Together, or Συναίρω To co-operate.

|| Κατά Opposite to, to make disappear.

SECTION IV.

OBSERVATIONS ON THE AURORA BOREALIS. BY W. STURGEON.

1. *September 29th*, 1828.—My attention was first called to this beautiful spectacle about eight o'clock in the evening. The sky was quite clear, and the stars shone with great brilliancy. The Aurora at that time exhibited a well-defined, highly-illuminated segment of a circle, the highest point of which was near to *Dubhe*, or the northern pointer (α *Ursæ Majoris*), and the extremities reached nearly to the horizon. The figure of this arch was similar to that of a rainbow, but its colour was that of a candle flame: it was soon observed to be changing its position and dimensions, by gradually expanding in every direction—the extremities stretching more towards the east and west points of the horizon, whilst the curve became proportionably elevated in the heavens. The whole of the arch, with the exception of the western limb, which continued extremely brilliant, became more feebly illuminated and less perfectly defined at its edges as it expanded. The diminution of the auroral light, however, was not regular, but varied in degree of splendour almost every moment: sometimes it appeared very feeble, at others quite brilliant. About a quarter before nine the light of the eastern extremity, which, from the first, had been the most feeble, began to be diffused, and to exhibit gentle, lambent, soft coruscations in the direction of the curve, but soon mixing with the "Milky-way," was lost in that luminous belt.

About this time, with the assistance of Mr. Marsh, the electrical kite was raised in the Artillery Barrack-Field, and about three hundred yards of wired string were let out. The wind was brisk and westerly. Strong sparks were taken from the reel of the string, and a jar was readily charged to a considerable intensity; but nothing particular, as regards the Electricity of the atmosphere, during a quarter of an hour, the time which the kite was afloat, was observed. The sparks and charges of the jar were neither more frequent nor more intense than is usual on such fine evenings, when no Aurora is present.

The coruscations of the Aurora were extremely faint, pale, and unfrequent; those embraced by the curve undulated gently downwards in the direction of radii, converging to a centre, the situation of which appeared to be in the magnetic meridian; whilst those that were observed to be exterior to the curve darted briskly in the shape of lambent streamers, and in the direction of tangents emanating from various points of the western limb of the glowing bow.

About a quarter past nine the arch had reached the zenith, but never passed southward of that point. It now became very faint with the exception of the western limb, which still continued bright, especially the western extremity near the horizon, which, although not steady, was constantly the most brilliant part of the Aurora. I had from the first expected to have seen a dense cloud, such as frequently accompanies the Aurora Borealis in the northern horizon; but, until the arch had nearly reached the zenith, no such cloud was visible. About this time, however, the anticipated associate began to make its appearance, which, for a while, was exceedingly faint and dim. Shortly it became more dense and better defined, having bright light bordering its upper edge; but no coruscations, though closely looked for, were observed to emanate from this part of the Aurora. The glowing curve which bordered the black space below appeared to be concentric with the expanded arch before described, and which was the southern boundary of the whole display. The central dark speck, although unusually transitory, was, during its short stay, scarcely one moment steady, ever varying in density, magnitude, and the sharpness of its boundaries; sometimes it was nearly lost in the horizon, whilst at others it darkened an extensive area, beautifully margined with a full glowing bow, which seemed like a *glory* of the painter, the centre of which appeared to be the centre of the whole phenomena.

When the larger arch had reached its highest elevation, the western extremity appeared to be about ten or twelve degrees north of the magnetic west, and the increase of curvature a little above the horizon gave it the appearance of a portion of a circle in perspective, with the upper edge above the eye, its centre above the horizon, and its plane sloping downwards towards the north.

About a quarter before ten, the Aurora became extremely languid, the coruscations had ceased, and the dark polar segment had completely vanished: a faint diffused light in the west was the only remaining trace of the once splendid celestial arch. Immense clouds soon appeared in the horizon, and shortly covered the whole hemisphere; and thus nature, as if mimicing man, dropped the curtain at the close of the scene, and darkened the airy stage from which had just vanished a most imposing spectacle. The electric kite was floated the following day, and the atmosphere was found to be more intensely charged with fluid than had been observed during the previous summer.

W. S.

2. *Woolwich*, *January 7th*, 1831.—A beautiful display of the Aurora Borealis was observed at this place on Friday night, January 7th, 1831. The Aurora commenced with the evening, and was very distinctly seen at about half-past five o'clock, exhibiting an arch of faint yellowish light, bordering a dense black area, which was bounded by the arch and the northern horizon. The Aurora became more brilliant as the evening advanced and got darker, darting occasional faint flashes of light upwards from the bright and comparatively steady luminous curve. About half-past six a second, and apparently concentric, bright arch made its appearance at a greater altitude than the former, and continued nearly the whole time of the remainder of the display. These two arches of light were frequently, after this time, very badly defined, ever varying in breadth, and softening gradually into shade, particularly at their convex edges, by the lambent streamers which gently played into the partially illuminated expanse above. These soft gliding streams seemed in continual play between the bright arches, flashing from the convex edge of the lower or innermost, and sometimes blending the two curves into one confused light, but never to that extent as to obliterate the distinction of the two luminous arches, which the eye could always trace by the superior refulgence of their light. Between nine and ten o'clock the altitude of the superior arch advanced from 20° to about 24°, but never ascended higher than that point. The inner or inferior arch advanced at the same time, and apparently in the same proportion, so that the *same* distance (about 10°) between the two curves of *strongest light* was *nearly*, perhaps *exactly*, preserved during the whole time. The extremities of these arches never completely reached the horizon, but were gradually lost in a dark gloom, resembling an exceedingly dense fog, although the atmosphere in every other part was perfectly clear. This appearance was particularly remarked on the eastern limbs, which were lost at various altitudes. The western limbs of the bright curves could not be so distinctly traced at the place where these observations were made, on account of their mixing with the reflected light in the atmosphere of the burning gas in London, which, at Woolwich, is always seen in the night as a bright cloud hovering over the metropolis.

About nine o'clock I called on Mr. Barlow, to inform him of the Aurora. Mr. B. however, had seen it all the evening. I remarked before I left him that the centre of the Aurora in the horizon was considerably to the west of the north, and near to the magnetic meridian, a circumstance which he had already observed. I immediately returned home, and having a very delicately suspended magnetic needle, I placed it in a suitable situation for observation, and so far neutralized the Magnetism of the earth as to leave no more power acting on the needle than was barely sufficient to arrange it in the magnetic meridian. I observed this needle, at intervals of two or three minutes, during the remainder of the display of the Aurora, but never detected the slightest change in its direction, nor was its repose in the least disturbed by any influence which I could ascribe to that phenomenon.

At half-past nine the Aurora increased in splendour, and shot its beautiful broad streamers upwards, as radii, from the external luminous bow nearly to the zenith. At a quarter before ten an immense faint stream of light kindled in the eastern extremity of the external bow, and flashed directly between the two large stars in the tail of *Ursa Major*, and in one moment described an arch of 100°. This streamer was not undulatory, but advanced gradually and steadily, leaving the whole of its track, for about a minute, in a steady glow of faint light; it then languished in every part, at nearly the same moment, gliding into still fainter light, and soon became entirely lost. About this time faint undulatory streams of light sprang from various parts of the central Aurora, and sometimes broad streaks of wavering light were seen glowing in the black area near the horizon. These latter displays, however, were not frequent, but on account of the contrasting blackness with which they were surrounded, appeared more brilliant than those flashes which occurred on the upper skirts of the Aurora.

At a quarter-past ten beautiful streamers were seen kindling upon the western limb of the superior arch of the Aurora, some of which instantly expanded into an attenuated light, which became extinct at a short distance from the point of their origin; whilst others, more permanent and brilliant in their display, stretched forth to an amazing distance in the heavens, and extended their lambent glow to beyond the planet *Mars;* but, like those which had before measured a vast arch of the heavens from the eastern limb, they, in a few moments, vanished for ever.

Soon after these displays of extensive streamers, there seemed a steady pause, as if the electrical powers which gave them birth had become partially exhausted. The steady light of the two concentric arches, with a few faint flashes about their edges, were the only traces of the Aurora. The cessation, however, was not of long duration, but the interval gave time for reflection. The night was calm and serene, not a breeze ruffled its repose, nor a cloud curtained any part of the heavens, save that dense black speck which seemed as a nucleus to the whole display of the Aurora Borealis. The atmosphere was cold and frosty, and the stars shone in all their splendour and glory. On turning towards the south, the spectacle presented to the eye was truly grand and imposing, and formed a most beautiful and striking contrast with the phenomena displaying in the north. *Taurus* had passed the meridian, preceded by the planet *Mars*, and *Orion* was now mounting the throne of night; refulgent *Sirius* blazed in the south-east of the stellar train, and enhanced the splendour and solemnity of the scene. In one part of the heavens was displaying the quivering blaze of a transient Aurora; in another, the sparkling light and steady march of a transcendent starry host: in the north, a splendid exhibition for the contemplation of the Electrician; in the south, those glorious orbs which are the objects of the Astronomer's research.

About half-past ten, the eastern limb of the Aurora again shot forth immensely broad streaks of light, with intervening dense shades. These streamers soon expanded,

and, mixing with each other, presented a steady uniform field of light. Other similar streamers darted upwards from the western limb, and, expanding like the former, heightened the illumination, which now extended to nearly half the concave of the heavens. The light vanished gradually, and was succeeded by faint streamers of much less magnitude. The dark space below the inner arch was now, for a short time, well defined by the bright glow round its upper edge, but it soon became confused and irregular. At eleven, a streak of bright light, like a yellowish cloud, stretched horizontally towards the east. In one moment after, a streamer kindled at its eastern extremity, and shot gradually upwards, passed the meridian, and terminated in a very faint light between *Aldebaran* and the *Pleiades.* About this time the undulatory streamers became beautiful and grand, playing in every part of the northern heavens to nearly the zenith, and on each side of the meridian to about the north-east and north-west points. Some bright coruscations occasionally flashed in this part of the display, and gave to it an exceedingly interesting appearance. A few moments dispersed these coruscations, which were succeded by a diffused faint light. The dense central darkness now suddenly disappeared, and a bright light illuminated the northern horizon for the first time since the setting of the sun. A dark broad streak soon stretched obliquely downwards, from east to west, nearly through the centre of the Aurora, and bright coruscations flashed in rapid succession from its upper edge.

About a quarter-past eleven the dark central speck again appeared, and some very bright streamers ascended from various parts of its upper or convex edge, which, as before, was now bordered by a bright steady light. Coruscations frequently about this time reached to between the pointers in *Ursa Major ;* they soon became very faint, and were succeeded by a dull steady light.

At half-past eleven the streamers became less frequent, the dense nucleus was ill defined, and the whole display began to languish. A bright curved light, however, with occasional ascending lambent streams, continued to direct to the general centre of the Aurora, which now appeared to approximate closer to the north point than in an earlier part of the display. The centre of the dark nucleus, however, was, from first to last, *west* of the north and very near to the magnetic meridian.

From twelve o'clock, nothing occurred worthy of remark; the splendour of the Aurora gradually declined, and at two on Saturday morning it had totally vanished. I observed, during the whole of the night, that the streamers, besides the vertical direction in which they generally shot, had also a horizontal motion from east to west; so that, in whatever part of the Aurora a streamer was kindled, it travelled slowly to the west, or towards the left hand of a spectator facing the north. It frequently happened that several were lighted up in rapid succession, each of which was always *west* of the preceding one. A meteoric star, which traversed the Aurora about ten o'clock, also fell sloping in the same direction.

W. S.

P.S.—On Saturday night, Jan. 8th, the Aurora was again visible. I saw it about ten o'clock. It exhibited no coruscations nor any flashes whatever. The only display was a broad arch of light, bordering the upper edge of a black area of the heavens in the north, and similarly situated to that which appeared the preceding night. At eleven o'clock no trace of the Aurora was to be seen.

3. *Woolwich, December 22nd*, 1834.—A beautiful Aurora Borealis was seen from this place last night. I was on Woolwich Common when I first saw it, then exactly six o'clock. It consisted of several groups of vertical beams of pale yellowish light, on both sides of the north star, extending nearly to equal distances in the western and eastern directions. These beams presented the strongest light at their bases, and grew gradually fainter to their superior extremities, where they softened and gently glided into the most attenuated light, and were lost at various altitudes, some of which were near to the zenith. These streamers soon faded, and gave place to a few straggling vertical coruscations, displayed in various parts of the northern sky, which, in their turn, were again succeeded by the finest streamers I ever beheld. It was now five minutes past six. These splendid streamers were of the same tint as the former, and extended from the black nucleus near the horizon to the zenith, in nearly the same manner; but the refulgence of these far exceeded that of the former. These streamers consisted principally of two parallel groups, one on each side of the north, and with some considerable distance between them. Smaller streamers were, however, playing in the intermediate space, and also on their outer horizontal skirts. The horizontal boundaries of the Aurora at this time seemed to be the "Milky-way," on the west, and near to the planet *Mars* on the east. From this time the Aurora gradually diminished in splendour, and, about seven, was nearly lost. It occasionally, however, brightened with a few faint flashing momentary streamers till between ten and eleven, at which time I discontinued my observations.

During the display of the fine streamers, which first presented themselves about five minutes past six, I hurried home to adjust a magnetic needle. It was about half-past six before I had my magnetic apparatus fit for observation, and the splendour of the Aurora had now passed its meridian. I diligently watched the needle and the Aurora till half-past ten, but observed nothing in the motions of the former that could possibly be attributed to the influence of the latter.

From the brilliancy of the Aurora at six o'clock, I imagine that it was exhibited at a much earlier period of the evening, but I have had no opportunity of ascertaining the fact from persons likely to have seen it. I think it is likely that the Aurora was very fine in Scotland, and perhaps in higher north latitudes, after seven o'clock—perhaps till nine or ten.

W. S.

P.S.—This Aurora appeared to have no partirular respect for the magnetic north: it was nearly, if not exactly, bisected by the *true meridian* during the whole of the time I observed it.

4. *Greenwich, November 16th*, 1835.—An Aurora Borealis of a very unusual character was seen in this neighbourhood, and I imagine over a large tract of country, on Wednesday evening, the 16th instant. I was walking from Greenwich to Woolwich between nine and ten o'clock, and when I had arrived at the top of Maize Hill, by the side of Greenwich Park, then about ten minutes past nine, my attention was first attracted by the fine light of the Aurora in the north. I walked on a little further till a good opening to the northern horizon presented itself from the road leading from Maize Hill to Mr. Angerstein's estate. At this opening I made a determined stand for the purpose of observing any novel phenomenon which the Aurora might happen to present.

At this time it consisted principally of a very extensive lateral range, on both sides of the pole star, of vertical streamers, which were pencilling the northern heavens from about 15° above the horizon to *Cassiopeia's Chair*, then about the meridian; and so uniform was their arrangement and splendour, that they presented one sheet of yellowish white light, the most intense at the base, and becoming more and more faint as they proceeded upwards, until quite lost at their terminal altitudes.

This appearance of the Aurora had but just stamped its impression on my mind, when in one moment the whole of the northern heavens appeared in one complete state of undulating commotion, heaving upwards in rapid succession immense waves of light,* which, like the streamers which preceded them, gradually diminished in brilliancy from their source near the horizon till their arrival at the zenith, which was their general vanishing point.

The horizontal range of the Aurora during this unusual display was eastward as far as *Jupiter*, whose azimuth from the north was then about 75°; and perhaps about the same extent westward on the other side of the pole star. I observed it stretch to beyond α *Lyræ* (*Vega*), whose azimuth from the north was about 60°, but could not very well ascertain the position of the western extremity at the place where I was standing, on account of the reflection of the gas light in London mixing with that of the Aurora, and the intervention of trees, &c.

This extensive ocean of light, which illuminated nearly half of the visible heavens, and whose waves rolled with the rapidity of thought, lasted about eight or ten minutes,

* These waves were seen at Milton, next Gravesend, by my scientific friend Mr. Swinny; and I beg to acknowledge the obligation I am placed under to Mrs. Swinny, who had also seen these waves, for a more happy description of them than any I had before thought of. They appeared to this lady as "waves of thin smoke or steam, behind which was placed a strong light." A more expressive description could not possibly be given.

perhaps longer, when the gradually began to disappear, and the Aurora to contract in all its dimensions. Until this time (nearly half-past nine), no dense nucleus had marked the centre of the Aurora: the stars were seen between the horizon and the luminous base as decidedly, though not so clear, as if no Aurora were present. The star *Benetnasch*, in the tip of the tail of the Great Bear (η *Ursæ Majoris*), was one of those which were observed below the Aurora; but Mizar (ζ *Ursæ Majoris*), then on the meridian below the pole, was seen in the bright arched base of the streamers and waves.

The last-mentioned change in the appearance of the Aurora brought it gradually to that state which is usually exhibited in some period or another of this boreal phenomenon. The dense black nucleus began to form, and soon curtained the stars which had previously twinkled in that segment of the northern sky. The luminous margin also, its usual attendant, became well defined, and its highest point was well marked to the westward of the meridian, perhaps nearly in the magnetic north. I now walked on, keeping the Aurora in view, which shot occasional streamers from various parts of the luminous arch. Just before I entered Woolwich, about ten o'clock, another fine display of vertical streamers spread over the northern sky, and continued for nearly a quarter of an hour. By this time I reached home, but too late to ascertain their effect, if any, upon the magnetic needle, for the faded away very rapidly after my arrival, and before eleven the Aurora had entirely disappeared.

During my walk home, I observed several fine meteoric stars, most of which appeared to be shot from the same point of the heavens, which point was somewhere in a right line between me and *The Twins*. One of these meteors shot with a moderate velocity across the north part of the meridian, at an altitude of about 80°, and appeared to traverse an arch of the heavens of 90° or 100°. It burst into several luminous fragments at the western termination of its range, and became extinct in a moment. I listened for some time, but heard no noise; neither did my servant, who was with me, and who listened attentively at my request. I had previously pointed out to him the direction he was to look in, and he saw the meteor from the first to its last appearance. He also directly afterwards saw another from the same quarter, which traversed the heavens in nearly the same direction as the former. He called out to me, but it was lost without my seeing it. These meteors were seen about five minutes before the last display of streamers mentioned above.

I saw no appearance of the Aurora to the south of the zenith, though frequently looked for. The sky was quite clear of clouds, and the black southern expanse, studded with its brilliant stars, afforded a fine contrast to the display of the Aurora in the north.

W. S.

P.S.—Whilst writing the above, a friend has called on me, who saw fine streamers about half-past eight o'clock.

5. *London*, *September* 16*th*, 1838.—The Aurora Borealis, after rather a long absence from this neighbourhood, has again made its appearance, and displayed its transient beams, twice at least within the last week. On Sunday evening, the 16th of September, I observed an unusually strong light among the clouds in the northern parts of the heavens, from which I suspected that an Aurora had either appeared in an earlier part of the evening, or that it was about to show itself. The light soon became much stronger, probably from the dispersion of the clouds, which had now mostly left the theatre of display; and the Aurora was identified by a few dim vertical streamers, that occasionally gleamed on the eastern side of the north point of the meridian. The general glare of light became much stronger in a very short time afterwards; and about nine o'clock, or perhaps a little after, undulatory waves of light rolled upwards in a very magnificent manner. At this time there did not appear any indication of the black segment which is usually seen beneath the brightest part of the Aurora; and the light was so strong, and generally diffused in the northern heavens, as to prevent ascertaining with exactness both the *source* of the waves and also the highest altitude to which they ascended. They seemed to come into existence at about 8° or 10° of altitude, and roll to elevations between 20° and 60°, and some of them might possibly ascend much higher than the latter altitude. The waves stretched horizontally across the meridian to between 40° or 50° east and west. They had very much the appearance of illuminated vaporous matter, and did not undulate at a very brisk rate. They were densest at their commencement, and became more and more attenuated as they ascended the heavens, and vanished at their terminal margins in the softest diffusions of pale expiring auroral light. The waves were succeeded by a few feeble streamers of considerable length, but without much motion, and mostly displayed on the western side of the meridian. It is very probable that, had there not been such a general glare of light in the northern part of the heavens, many more streamers would have been seen.

The sky became quite clear of clouds a short time after the disappearance of the waves, and the central part of the Aurora had obviously taken a position nearly in the magnetic meridian—although, previously to the appearance of the waves, the region of strongest light was as obviously on the eastern side of the true meridian.

W. S.

6. *London*, *September* 3*rd*, 1839.—A very singular Aurora Borealis appeared at London and its vicinity last night. It first made its appearance about a quarter before nine o'clock, and continued nearly the whole of the night. I was walking from Brixton to Peckham, between nine and ten, and kept the Aurora in view the whole of the time. I first saw it when passing Brixton Church, then about nine o'clock; its appearance was that of a yellowish light, at a small altitude above the northern

horizon. In the course of a few minutes, a few faint, straggling streamers glided upwards to a considerable height; and soon afterwards several groups of brilliant streaks of red and white light shot over an immense track of the northern heavens, to nearly the zenith. Besides these streamers, there were also splendid blushes of alternate *stationary* and *moving* red and white light. The sky was partially covered with thin, vapoury clouds, which had an obvious influence on the colour, and the apparent horizontal motion of the light, which was easily discerned to be behind or beyond these thin clouds of vapour, and assumed a deeper tinge of redness as the vapour became more dense between it and the spectator. As this was the first time of my observing a red light during the display of an Aurora, I became very anxious to know its cause, for I never yet saw the electrical light, in artificially-attenuated air, anything like the colour of the light which I observed on this occasion. It was sometimes of a deep crimson, at other times of an almost fiery red; then pink, very light pink; next the yellowish white colour which the Aurora most usually displays; and so on for several alternate successions. At other times the Aurora would seem to reverse the order of colours, beginning with the ordinary white light, and passing through the different red tints down to the perfect crimson, and then return gradually to the ordinary white.

I had several opportunities of observing these curious changes in the colour of the light before I arrived at Camberwell. Just before I entered the Grove, Camberwell, then about half-past nine, the northern sky was illuminated throughout an immense horizontal range, with a splendid red light; but when I arrived at the Church-yard, about five minutes afterwards, the red light had nearly disappeared, only a small portion remaining on the northern edge of a thin fleece of vapour, at a considerable altitude above the north-western horizon, being succeeded by several splendid groups of the usual white streamers. From this time till a little before ten, the Aurora languished very considerably; but, about five minutes before ten, it re-appeared with all its former splendour, with the exception of the red colour. This last sudden display presented many exceedingly fine groups of intense streamers, which shot upwards to the zenith, and covered an immense space in the heavens; but lasted only a few minutes before they vanished, and appeared to leave the night in comparative darkness. I watched the Aurora till about half-past ten; but, as I could see no indication of its continuance much longer, I ceased my observations. I have been told, however, that the Aurora re-appeared in great splendour, and continued until three o'clock next morning.

I never before observed an Aurora Borealis expand to so great a horizontal range as that I have now partly described. *Lyra* and *Capella* were excellently situated for giving a good idea of the horizontal extent of the Aurora, the former star being just within its western, and the latter just within its eastern margin. The thin vapoury

clouds presently clearing away, these two conspicuous stars were afterwards noticed to be within the limits of the auroral beams. Before ten o'clock, the sky had become pretty clear, and the stars shone in every part of the visible heavens. I did not observe any meteoric stars. W. S.

7. *Manchester, October* 16*th*, 1840.—An Aurora Borealis of considerable magnitude and brilliancy, but attended with no peculiarity, was seen here from seven till eleven o'clock last night. It consisted principally of a strong steady light in the northern heavens, with the usual black, foggy nucleus below; and of many fine streamers, which were displayed at different times during its appearance. The colours of both streamers and of the stationary light was of a misty white colour.

W. S.

8. *Manchester, December* 22*nd*, 1840.—A very brilliant Aurora Borealis was seen in Manchester on the evenings of Sunday and Monday last. I happened to see this phenomenon last night, about ten o'clock. It was then very bright, with an immense quantity of diffused white streamers. This, I understand, was the general appearance of the Aurora on the preceding evening. The wind was very light and easterly, with a gentle frost. The Aurora Borealis was seen here several times in January last; on one occasion the upward waves of light were very grand. W. S.

9. *Manchester, April* 5*th*, 1843.—Yesterday the sky was completely covered with one sheet of dense cloud during the fore part of the day, and a smart north-west wind prevailed till nearly the evening. In the afternoon the lower stratum of cloud cleared away, and fine groups of well-defined clouds were seen floating in a higher region of the air. Towards sunset every vestige of cloud had disappeared, and the wind subsided to a mere zephyr.

As the sun disappeared below the western horizon, I observed an arched band of pale light in the northern heavens, having its extremities resting on the horizon, and its apex, or highest point, nearly, if not exactly, beneath the pole star. As the evening darkened, the luminous band showed itself to be an Aurora Borealis, of an immense magnitude, both in breadth and in the span of its arch; and had not the moon's light interfered, the phenomenon would have been splendid indeed. And although the moon, then six days old, shone with great brilliancy, and was situated not far above the western boundary, the Aurora was remarkably perfect from one end of its arch to the other; which, at eight o'clock, reached (as nearly as I could estimate) from between 10° and 20° of *Pisces*, on the west, to about the same distance within *Ophiucus*, on the east. Its greatest altitude, at that hour, was about the same as that of the star *Deneb*, in the tail of the *Swan*, but somewhat eastward of it. It

passed through the *Harp*, and the star *Vega* shone beautifully through the auroral light, which was more resplendent in that quarter than in any other, not only at that time, but during the whole period of display, till eleven o'clock at least, at which hour the Aurora had considerably faded.

Although, from the moon's light, the few streamers that were observed were very faint, those in the eastern limb of the arch were by far the most frequent and luminous: and, during the greatest part of the evening, the centre or highest point of the Aurora, was obviously on the *eastern side* of the meridian.* But, about ten o'clock, the highest point seemed to have got to the *western* side of the north. The arch, however, had expanded so much in breadth before that time, and its light had become so diffused, that it was difficult to ascertain the exact position of the highest point. About half-past ten, *Cassiopeia* wheeled into the auroral arch, which soon embraced the whole of that constellation. The stars forming the chair were distinctly seen in the maze of light. The eastern limb of the Aurora had now spread both upwards and downwards, so as to cover that part of the heavens from nearly the altitude of *Vega* down to the horizon. The star *Deneb* was immersed in it, and shone remarkably clear, though not quite so brilliantly as if the Aurora had not interfered. It was partially tinged with the auroral colour, which was that of a yellowish white flame.

The northern boundary of that portion of the auroral light which, in the eastern limb, shot downwards, was a tolerably well-defined right line, sloping *northerly downwards* to the horizon, and appeared as if directed to the centre of a circle, of which the arch was a segment, which must have been considerably below the horizon. Indeed, all the streamers that I saw, seemed to be radii diverging from such a centre.

During the display of this beautiful Aurora, several thin light clouds formed in front of it, some of which moved off slowly with the light wind, whilst others disappeared nearly at their respective birth-places, as if again dissolved into invisible vapour. In the eastern limb of the Aurora a fine black cloud made its appearance, and continued for a long time at nearly the same spot, with its northern extremity between the Aurora and the eye of the observer. It curtained a considerable portion of the Aurora from view, and formed a beautiful contrast with the bright marginal light which proceeded from the more remote meteor.

The appearance of these clouds formed an important feature in the spectacle, by their proving that the Aurora was at a much greater distance than themselves from the place of observation. About eleven o'clock the moon, which had shone beautifully all the previous part of the evening, became bordered with a dim beard, and encircled by an imperfect halo. The former increased in density as she neared the horizon, and her light gradually impaired by the interruption of atmospheric haze. W. S.

* I have observed, on previous occasions, that the highest point of the Auroral arch is *eastward* of the north during the early part of its appearance, though it took a *westerly* declination afterwards.

10. *Kirby Lonsdale, Westmoreland, Sept. 29th*, 1847.—A beautiful display of the Aurora Borealis was seen here on Wednesday night, Sept. 29th. The weather had been very fine during the whole of the day, and nothing unusual was observed till after eight o'clock at night. Between eight and nine, however, a bright space in the heavens was observed to border the northern horizon, and soon afterwards a black curvilinear cloud encompassed its upper edge, having on its convex or upper margin a bright glowing segment of a circle, concentric with the cloud within it. About nine o'clock, a few faint streamers shot upwards from the glowing bow. These first began on the eastern extremity of the bow, but shortly afterwards, from every part of it, increasing in splendour for several successive minutes, when suddenly the whole of the Aurora disappeared, as if extinguished by magic. The phenomena did not, however, terminate here: it seemed that the curtain dropped merely to show the *end of the first act*. In a few minutes a new piece was introduced, the character of which was not only very different to the former, but far superior in brilliancy and grandeur. The northern skies were now lighted up with a rapid succession of luminous waves, rolling upwards from nearly the horizon to the pole star. This display commenced a little after nine o'clock, and lasted till nearly ten. About the latter hour the brilliancy and frequency of the waves began to languish, and in less than half an hour afterwards the Aurora had entirely disappeared, and was not seen to re-appear during the night.

On the following night the Aurora Borealis appeared again; but, in consequence of clouds, could only be seen at intervals, when they partially cleared away between the meteor and the observer. No streamers were observed, nor any other indication of an Aurora beyond that of a strong light. A phenomena of this kind frequently appears the first night, and sometimes the second night also, after a grand display of the Aurora Borealis.

I have learned from the Rev. Mr. Abbot, and from some other gentlemen in this neighbourhood, that the Aurora Borealis has been frequently seen within the last two months. Since that last described, there have been no less than four or five displays of the meteor observed by myself. Last night (Oct. 13th) we had indications of a grand Aurora as early as the setting of the moon, or shortly after sunset. It turned out, however, to be but a very humble display, at least during my observation, which was till ten o'clock. A great number of streamers were observed, but their light was very feeble. The horizontal range of the Aurora was very extensive, but where its centre was situated I could not ascertain, because of the haze of light in which the streamers were seen. I have heard from a friend to-day that the Aurora was very grand between twelve and one o'clock this morning. W. S.

11 *Biggins, near Kirby Lonsdale, October 24th.*—On Sunday night last, the 24th instant, we had another grand display of the Aurora Borealis, attended with peculiari-

ties of a remarkable character, and such as are but seldom witnessed. The Aurora was first seen as early as a quarter before six in the evening ; at which time, and during an hour afterwards, streamers of considerable magnitude and of different colours were seen to shoot upwards, from a low altitude in the heavens to the zenith of the observers. The grandest display of these streamers occurred about half-past six o'clock. They were of various hues—some crimson, some pink, others yellow, green, and violet. The largest of the whole appeared like an immense pillar of crimson light, situated about the north-west. The others were generally assembled about the true north ; the whole had a slow *lateral* motion from *east* to *west*, across the northern part of the meridian.

On Monday I went to Cantsfield (about four miles south of Kirby Lonsdale), where I heard of a still more extraordinary phenomenon than anything that was seen at Kirby Lonsdale or Biggins. About ten o'clock on Sunday night, there appeared in the west, or rather a little south of the west point in the heavens, a fiery coloured light, which stretched from a broad base at the horizon to nearly the zenith. It soon became of a deeper red, approaching to crimson, and commenced a motion towards the north-east ; and eventually, after being seen for about half an hour, it disappeared altogether in that quarter.

No cloud was present during the whole time, and the moon shone with her usual lustre in a clear sky.

Some splendid meteoric stars fell during the time of observation. Much distant lightning was seen during the evening, both at Cantsfield and at Kirby Lonsdale.

I will here beg to remark that large meteoric stars have been very frequent of late. Many of them have traversed a long arch of the heavens, and eventually have burst into a number of glowing balls, very much in the manner of sky rockets. Lightning also has frequently been seen, in the evenings, within the present month.

W. S.

12. *Biggins, near Kirby Lonsdale, November 1st*, 1847.—Last night, I had a fine view of the Aurora Borealis, from this village. It consisted of an immense horizontal range, perhaps extending 140°, of streamers which shot their soft light slowly upwards, from a segment of fog (it could hardly be called a cloud) which was illuminated on its upper edge, and occasionally in every part of it. The streamers were well defined, broad but not lofty. The light of which they were composed was unusually soft and pleasing to the eye. Above the streamers, and sometimes amongst them, there appeared a crimson light hovering in the sky. It had no appearance of being an *original* light, but looked more like the red component from a decomposition of the natural white light. The stars were seen through it, though they were not so bril-

liant as during its absence. I have every reason to think that not only the various tints that are sometimes seen in the Aurora, but also some other of its characteristics, are the effects of refraction and reflection of the original auroral light; or, in other words, that they are *secondary* effects. W. S.

NOTE.—The dates of the preceding displays of the Aurora Borealis, are those given in the *heading* of each individual case. The dates of the several descriptions of them will be found in the Abstracts, Section II.

A DESCRIPTION OF SEVERAL EXTRAORDINARY DISPLAYS OF THE AURORA BOREALIS, AS OBSERVED AT PRESTWICH* DURING THE WINTER OF 1848-1849; WITH THEORETICAL REMARKS.

(Read at the Royal Institution, Manchester, March 28th, 1849, and originally published in the Edinburgh New Philosophical Journal.)

Having had opportunities of observing several fine displays of the Aurora Borealis since the commencement of last autumn, some of which presented phenomena of very rare occurrence, a description of them as they appeared at this place can hardly fail to be interesting to philosophical inquirers; more especially as data are still wanting to establish a foundation for a true theory of the meteor—a physical problem of long standing, and hitherto without any satisfactory solution.

The first grand display of the Aurora Borealis, in this list, occurred on Wednesday evening, October 18th, 1848. It began with the close of the day, and lasted, with various degrees of brilliancy, till ten o'clock, or probably later; for, labouring under the effects of a severe cold, I could not watch it closely out of doors. It consisted of an extensive arch of light, which crossed the magnetic meridian at nearly right angles (which, however, was not its invariable position, but that which it assumed during the greater part of the display), and immense floods of lambent streamers, which occasionally flowed gently upwards and downwards, from various parts of the arch. The average colour of the light was that of a candle flame, though in parts, and especially towards the eastern extremity, the colour was red, inclining to violet. I observed nothing extraordinary in these streamers, nor in the general aspect of the Aurora; but, for reasons already stated, I could not make a minute survey.

For several days previous to this Aurora, the atmosphere had been highly charged with the electric fluid. On the preceding Saturday I had the electrical kite elevated about 400 yards, from the string of which a small jar was rapidly and frequently charged: a steel needle was magnetized, and its poles reversed several times, by the

* Prestwich is a village at the distance of four miles from Manchester, in a north-west direction, on the New road to Bury, from which it is also four miles distant.

discharge of the jar, and also by sparks direct from the kite string. The magnetic polarity of the needle indicated a *downward* current in the string, which was the case in other experiments on several days previously, though not to the same extent of power.

This Aurora was observed at many places wide apart, which showed that it occupied an immense space in the heavens. It has been differently described by different observers, to whom it appears to have presented different aspects. The brief description given above is copied from my journal, the particulars being written down on slips of paper as the phenomena occurred, and afterwards copied into the journal, which is my usual mode; for it is next to impossible to remember all the varied features which the meteor presents during the several hours that is sometimes required to watch its manifold and rapid transformations.

The next display of the Aurora Borealis, of any consequence, occurred Oct. 27th. During the morning and all the forenoon, we had continuous rain, which cleared off about two P.M. I had been looking out for the Aurora all the evening, and about six o'clock an arch of dim light appeared in the northern heavens. It was very low, and not of that extensive horizontal span occupied by the Aurora of the 18th instant. The western extremity reached a little westward of *Arcturus*, the star being much higher than the auroral arch; but, as it was fast descending towards the horizon, it passed through the arch, whilst the latter remained stationary, or nearly so. Some fine groups of lambent streamers occasionally flowed upwards from different parts of the glowing bow; and also another feature which the Aurora displays—the gentle blushes of pale soft light were frequently seen in the dim haze that almost invariably accompanies the Aurora Borealis. Between seven and eight o'clock, a few straggling clouds came floating across the Aurora. Such interruptions to the observer's view are exceedingly interesting events; for they never fail to show that the auroral light is at a greater distance from the place of observation than the clouds themselves, which is one step, at least, gained towards obtaining a true true theory of the cause of the meteor; but should nothing further be ascertained, by the interposition of these clouds, than the locality or region of the atmosphere in which the Aurora is situated, it would be the means of setting at rest an inquiry of great interest concerning the real height of the meteor.

There are other features occasionally conspicuous in the Aurora Borealis, which have long been noticed and recorded as the most astonishing appearances of the whole. I allude to the colours that sometimes adorn the meteor. They have for a long time appeared to me to arise from a decomposition of the true auroral light (white light, or rather that of a soft pale candle flame), accomplished by refractions and reflections amongst the abundance of aqueous particles hanging in the regions of air where the electric fluid is in motion, or between those regions and the eye of

the spectator. There can be no doubt of the electric origin of the Aurora Borealis, since many of its characteristics can be beautifully imitated by the electrical apparatus. The violet tint is easily produced by an electrical discharge through highly attenuated air; but the green, the blue, the orange, the yellow, and the deep red cannot be imitated by any form of electrical experiment hitherto known, in which the light is shown in common air, however much it may be attenuated. But these colours may easily be accounted for under the supposition of an abundance of aqueous vapour in the regions of an auroral display—a concession by no means unreasonable when we take into account the season of the year (from about the autumnal to the vernal equinox) in which such spectacles are most frequent, the hazy appearance of the sky at the time, and the occurrence of wet weather that usually follows.

By looking over my journal for several years past, I find that the grandest displays of the Aurora Borealis have been closely followed by wet weather; and the following extract from the description of an Aurora which I observed in the vicinity of London, on the evening of September 3rd, 1839, will probably appear more eminently calculated to develop the true character of the spectral colours accompanying the Aurora Borealis than any other attempt at explanation hitherto on record. The observations were made whilst walking from Brixton to my residence in Pomeroy-street, Old Kent Road:—

"The sky was partially covered with thin vapoury clouds, which had an obvious influence on the colour, and the apparent horizontal motion of the light, which was easily discerned to be behind or beyond these thin clouds of vapour, and assumed a deeper tinge of redness as the vapour became more dense between it and the spectator. As this was the first time of my observing a red light during the display of an Aurora Borealis, I became anxious to know the cause, for I never saw the electrical light, in artificially-attenuated air, anything like the colour of the light which I observed on this occasion. It was sometimes of a deep crimson, at other times of an almost fiery red; then pink, very light pink, next the yellowish white colour which the Aurora most usually displays, and so on for several alternate successions. At other times it would seem to reverse the order of colours, beginning with the white light, and passing through the different red tints down to a perfect crimson, and then return gradually to the ordinary white. I had several opportunities of observing the curious changes of colour in the auroral light before I arrived at Camberwell. Just before I entered the Grove, at Camberwell, then about half-past nine o'clock, the northern sky was illuminated throughout an immense horizontal range with a rich red light; but when I arrived at the Church-yard, about five minutes afterwards, the red light had nearly disappeared, a small portion only remaining on the northern edge of a thin fleece of vapour, at a considerable height above the western horizon, being succeeded by several fine groups of the usual white streamers."*

* Aurora Borealis, No. 6, page 497.

On this occasion, I had an excellent opportunity of observing that the red light never appeared when the sky was pretty clear of those thin vapoury clouds which frequently skimmed across the Aurora, and I eventually became so perfectly convinced of the effects they produced on the colours of the light, that I could predict the appearance of the red colour, by observing the approach of the thin fleeces of vapour before coming within the limits of the Aurora. From these facts, it would appear that the prismatic colours which occasionally adorn the Aurora Borealis are *secondary* phenomena, produced by the ordinary decomposition of the original light, and need not be looked upon as anything extraordinary, beyond the certainty of an abundance of aqueous vapour, either in the region of the electrical disturbance or at a lower altitude in the atmosphere.

But to return to the Aurora of October the 27th, the luminous arch, by admeasurement, never exceeded 12° of altitude, its highest point being close upon the magnetic north of this place. Its horizontal span was about 105°; but in consequence of the diffused character of the light constituting this principal feature of the Aurora, these dimensions can only be considered as close approximations to the truth. Before nine o'clock, dense clouds shrouded the Aurora from view; and, as the sky soon became covered with clouds, the spectacle closed for the night. I had a magnetic needle delicately suspended by a single fibre from the cocoon of the silk-worm, which was closely watched at every opportunity during the whole time. About half-past seven, it became slightly agitated, but made not any excursion either eastward or westward—its motions being a mere nodding in a vertical plane, which was continued for some time, probably much longer than the cause continued to operate upon it, as is always the case when needles are thus delicately suspended.

The next grand appearance of the Aurora Borealis was on Friday night, November 17th. A strong south wind, with heavy rain, began about six in the morning and lasted till ten in the forenoon, about which time the wind veered westward until it arrived at north-west, and the rain cleared away. The wind still continued high; the thermometer was 50°; slight showers towards evening; and the night was introduced by the most extensive Aurora Borealis I had ever beheld.

It began with the close of the day, and lasted all night. I observed it till twelve o'clock, at which hour, though cloudy and scarce a speck of clear sky to be seen, the whole canopy was illuminated by the Aurora; and the light generally, even in the southern regions of the heavens, was much stronger than that afforded by a *thinly-clouded* full moon.

I was in Manchester at the time of its commencement, and had no opportunity of seeing it till after my arrival at Prestwich by the omnibus, about a quarter before eight o'clock. At that moment my attention was attracted by the unusual glare of light; and, on looking up, I perceived immense floods of streamers flowing in almost

every direction, and on every side excepting the south, which appeared totally devoid of them, though strongly illuminated. The east and west parts of the heavens appeared the most luminous of the whole, although all around the north was in a blaze of hazy streamers: indeed, every part of the Aurora appeared as if composed of illuminated aqueous vapour, distributed in various forms in different parts of the circumambient aerial space. From the east, round by the north, to the west, these streamers appeared to flow *upwards* in the usual way; but in the zenith, and all about that point, they arranged themselves directly across the meridian; and on both wings of this line of streamers, and *southwards*, the auroral light consisted of fine steady blushes, without any attendant streamers whatever.

Light fleecy clouds were passing over, with a brisk north-west wind, at the time, and some rain had fallen whilst I was in the omnibus; but every streak and other form of cloud—all of which were exceedingly thin or attenuated—seemed to take a part in the auroral display; and those parts of the heavens which were not covered with cloud appeared as if full of a luminous mist or haze, through which some of the principal stars were seen.

Such was the state of the Aurora Borealis when I arrived at home, then about a quarter before eight o'clock; but I was soon made to understand that I had lost the grandest part of the spectacle, which, as I was told, occurred about seven o'clock. The Aurora had been watched by my family from about six in the evening, at which time the streamers were very fine; they occupied an extensive lateral range, and were of the usual pale colour. A little before seven the streamers became more abundant, intensely brilliant, and reached over the zenith southwards to the distance of 20° or more; and what increased the splendour of the scene, was a brilliant crimson canopy in the heavens, which became gradually transcoloured into a lively purple. On the eastern side also, about 15° above the horizon, was an immense blush of red light, which gradually faded away and was lost.

These were the principal features of the Aurora till about seven o'clock, after which hour its appearance was nearly the same as when I first saw it. Shortly after eight an abundance of detached clouds floated over this locality, and partly obliterated the splendour of the meteor, which was now only occasionally exhibited in the openings amongst them. The wind being brisk, the groups of clouds that passed over made a quick transit, and soon gave place to a full display of the auroral glare, which, though strongest about the northern heavens, spread more or less over every part of the celestial vault.

Before nine o'clock the sky was again completely covered with thin clouds, but still a strong light passed through them, which gave a distinctness to objects and to boundaries of land, as though it had been the twilight of a fine evening. About ten, an extensive blush of red light hovered in the southern parts of the heavens, at an alti-

tude of about 40°, and continued nearly stationary for several minutes, with every appearance of the usual aerial spectre of an intense conflagration below. The curtain now dropped till nearly eleven, when an intense light in the east and west, with a few streamers in the north, burst into view as if by magic; for thin clouds still obscured the stars, except at occasional openings, where they were seen as bright spangles behind a luminous mist.

In one of these openings an extensive blush of fiery red light appeared in the west, and gradually floated southwards, along with the group of clouds that surrounded it, until it reached a little eastward of the southern meridian, where it appeared to remain stationary for a short time, gradually diminishing in intensity and dimensions till it finally disappeared. From this time till twelve o'clock nothing remarkable was observed beyond a strong glare of light which pierced the clouds and illuminated the whole expanse of country beneath.

The seven stars and *Aldebaran* were immersed in a strong auroral light at midnight when the clouds had partly cleared away. The luminous bow, or arch, which frequently attends the Aurora Borealis, never appeared during any part of the scene.

An Aurora Borealis appeared on the 18th of November, which consisted of a glare of light in the north, attended by a few shooting streamers.

Tuesday, November 21st.—The morning was gloomy, with a light west wind. The thermometer stood at 40°, but rose to 44° in the forenoon. The afternoon was cloudy, with light showers of rain, and a brisk west wind. The sun set very red, and gave a glowing redness to the vicinal clouds, which were arranged in bands, in the direction of the wind, from west to east, and shortly afterwards traces of an Aurora Borealis were depicted in the heavens.

The usual auroral bow appeared in the north before six o'clock, and at a quarter past six bands of streamers appeared in various parts of the heavens; and at the same time a broad field of red light hovered, for about two minutes, in the north-east. The bow, at this time, was not so high as the lower pointer in the *Great Bear*. At half-past six the arch, or bow, reached to an altitude of about 15°, but was very imperfectly formed; it became more and more diffused, spreading its light, in a short time, till it nearly covered the whole of the *Great Bear*. A few short streamers appeared at a low altitude in the north, as if they proceeded from a thin streak of cloud which crossed the meridian beneath, and parallel to, the luminous arch.

About seven o'clock, several bands of dim red light started from the west, passed through the zenith, through *Cassiopiea*, and extended to the eastern horizon; they were obviously illuminated streaks or bands of vapour arranged by the westerly wind. There was scarcely any auroral light in the north at the time, and the stars looked very dim. At a quarter past seven, a broad band of dim redness formed from west to

east ; it passed through the zenith and *Cassiopiea*, and travelled slowly southward until its eastern limb passed over the *Pleiades*. It broke up within three minutes after its formation ; the eastern part quite vanished, but the western limb continued visible for a much longer time—it was always broadest and brightest on the western side. This phenomenon was immediately succeeded by a band of dim whiteish light, which stretched immediately across the northern heavens, having a horizontal span of 120°, and an altitude reaching that of the *Great Bear*. About this time the whole of the northern parts of the concave seemed to be filled with broad dim bands of a smoky reddish colour, which had every appearance of being illuminated bands of vapour arranged by the wind, at this time very feeble. Several streaks of white light, at a higher altitude but parallel to the former, were also observed, all of which were arranged in the direction of the wind.

At about ten minutes before eight, a number of flashes of dim white light shot across the heavens in various directions, and lighted up the streaks of vapour as they came at them in succession. At eight o'clock the thermometer had fallen to 42°, and there was a dead calm. At half-past eight several broad patches of feeble white light passed swiftly along the sky, in almost every direction ; these were succeeded by a dim luminous white haze that seemed to fill all the northern heavens up to the zenith : it gradually waned, and in a short time finally disappeared, closing the scene for the night.

Sunday, *December* 17*th*.—Fine frosty morning with some fog. The day turned out fine, and the thermometer rose to 36°. At night we had a grand display of the Aurora Borealis. It began before seven o'clock, and lasted till after midnight. At half-past seven the Aurora consisted of three large patches of light, one in the east, one in the west, and the other just beneath the north star. Streamers shot from all these occasionally, but the principal light was in the west. About a quarter before eight o'clock an extensive range of streamers burst out all around the northern heavens, from the west to the east points, some of which assumed a dingy red hue for a moment, and then changed again to the soft white. There were two ranges of these streamers, one considerably higher than the other, with an unilluminated vacant space between them, though at first view the whole seemed to be but one group. The streamers of the uppermost range reached the altitude of *Cassiopiea*, at that time a little westward of the meridian. Between eight and nine o'clock, several flashes or waves of light swept across the zenith, and many other parts of the sky, in different directions. Several of these waves proceeded from west to east; many from north to south, some of which reached the seven stars; and others traversed the heavens obliquely to the former—all denoting an electrical disturbance in the higher regions of the air, illuminating highly attenuated vapour in which it took place. These flashes or waves were of precisely the same character as those which appeared on the 21st of November, but their transit was much slower, so that the eye

could follow them in their progress to their apparent destination. During this period of the Aurora, several light rain clouds scudded across from south to north, at a low altitude, and obscured the waves of light, evincing, as on other occasions, that the electrical meteor was above the clouds.

About nine o'clock, the whole of the celestial concave partook of the auroral scene, which exhibited very different aspects in different quarters. The northern parts had now become the brightest, though continually changing in intensity and tint of colour, the latter varying between a soft yellowish white and a dingy red. In the south there appeared nothing but a lurid red haze, which gave a dimness to the stars, though some of them occasionally shone without any perceptible interruption. At one time *Orion* appeared as if completely covered with a flimsy mantle of a deep red colour; so flimsy, indeed, that the principle stars, *Betelgeuse*, *Bellatrix*, and *Rigel*, suffered but little from their usual splendour: the natural red tint of the former, however, was obviously enhanced by the auroral haze, and the others slightly partook of the flimsy red tinge. Indeed, the whole of the stars in the southern heavens were more or less dimmed, and many of the smaller ones completely obscured. In the north also, and indeed on every side, a thin haze prevailed in obstructing the natural refulgence of the stars, rendering them dim and gloomy. There was a brisk south wind all the evening, and the thermometer stood at about 34°. The whole display of the meteor on this occasion, and also on the 18th of November, appeared to take place in an atmosphere of highly attenuated nubiferous matter.

Sunday, January 14th, 1849.—Stormy morning of west wind and heavy rain; the thermometer 50°. Very windy all day, with heavy showers; a loud clap of thunder about noon, which was heard for several miles round this place; and in the neighbourhood of Warrington, where there were several flashes of lightning seen, accompanied with loud thunder. At three in the afternoon, the thermometer fell to 43°, and to 40° at night. The clouds entirely disappear in the evening, and the stars shone with a feeble lustre, indicating a great abundance of aqueous vapour in the air.

About half-past eight a beautiful Aurora Borealis presented itself in the shape of a well-defined luminous arch, which crossed the northern heavens, and from which proceeded various groups of streamers; but nothing extraordinary was observed, though closely watched till eleven o'clock. The arch in this case was nearly, if not exactly, at right angles to the true meridian.

Monday, February 19th.—Stormy west wind, with heavy rain clouds in the morning; thermometer 47°: it rose to 50°, and much rain fell during the day. The wind continued high until evening, when it slackened a little, but still kept up a strong cold breeze. At night there appeared an Aurora Borealis of the most extraordinary character hitherto recorded in the history of the meteor.

It commenced with the close of the day, with a strong glare of light in the northern heavens, but without any definite shape or boundaries; and continued in this condition till nearly eight o'clock, about which time some faint colourless streamers appeared, and occasionally dim flashes of light swept across the sky, generally from east to west, and at a higher altitude than was reached by any of the streamers; none of which appeared to have any reference to the northern glare of light, which continued nearly steady from first to last. The horizontal span of this light reached from beneath the tail of the *Great Bear*, or about the shoulder of *Bootes*, on the eastern side, to nearly the chest of *Pegasus*, on the western side; but the boundaries were so badly defined that no exact points in the heavens could be selected to mark the precise dimensions. The altitude of this northern light was quite as difficult to ascertain as its horizontal range, because of its gradual softening into the ordinary nocturnal colour of the sky. I can only say that it embraced α *Lyræ* and α *Cygni*, which were seen within it—the latter star just within its upper edge.

Such were the characteristics of the meteor till nearly nine o'clock, about which time commenced the first novelty in the history of the Aurora Borealis. A glow of light made its appearance close to the tail of the *Great Bear*, which waxed to a considerable degree of brightness, and after remaining for about half-a-minute it gradually waned in splendour until it finally disappeared, This spectacle had just ended when a horizontal arrangement of short glowing beams, of the usual shape of streamers, began to parade the northern heavens, about half way between the steady glare of light, already described, and the pole star. They came into existence on the eastern side of the meridian, and marched very orderly, one after another, westward, in the same regular order of succession as they sprang into existence, until they reached a point directly beneath *Cassiopiea's Chair*, where they became extinct, and were successively lost in the sky at the moment of their respective arrival at this spot, their apparent destination. This scene lasted several minutes, almost without interruption; during some part of the time the line of columns, between the two points in the heavens, was complete from one to the other, and had very much the appearance of an army of soldiers marching in single files, where the observer could just see them coming into view on his right, and vanish on his left, the whole marching past as if for his especial review. The bases of these luminous beams were flat and well defined, but the upper extremities were of a diffused radiant character, and gradually softened off till lost. During the time of this strange spectacle, several minor groups made momentary displays in different parts of the northern sky, and all seemed to move in the same direction, from east to west.

The next scene in the drama was partly similar to that just described, but of far greater splendour and extent. It began about half-past nine, at a point near the tip of the tail of the *Great Bear*, with a steady glow of pale light, from which issued

an immense host of bright glowing beams, which marched across the meridian, with their centres at the altitude of the pole star, until they reached nearly to *Venus*. The movements of this grand array were slower than those of the last described columns, and also different in character. The first glided smoothly along without much vibratory motion; but these exhibited a kind of dancing or jog-trot sort of march, which appeared as regular as the march of an army of soldiers guided by a band of music; all hands, from front to rear, *keeping step* in a very orderly manner. The length of these beams much exceeded the length of the former group, and their upper extremities were so well defined that they formed, whilst the line was complete, a beautiful arch, the highest point of which was considerably higher than the pole star; but their lower parts shot downwards in variously-pointed terminations, like a series of *inverted* streamers, and were lost at many different altitudes amongst the stars, but never reached so low as the northern glow of light. Like the former group, these beams or columns sprang into existence individually, and in regular succession, from the same source near the tail of the *Great Bear*, and took up the line of march from the commencement of their respective births; so that the individuals forming the moving line were every one of a different age—the foremost the eldest, and all the rest in the order of succession from front to rear; and they appeared to vanish in the same order of succession at a point in the heavens close upon the planet *Venus;* so that the last which sprang into existence in the east kept its position in the rear of the line, all the way to the west, and was the last that was seen individually in this part of the aerial spectacle. But this was not the conclusion of the scene; for, instead of these luminous beams vanishing entirely in the manner of the previous group, they seemed to assemble in a close compact body in the west, where they disappeared as individuals, and to form a broad luminous streak, which reached downwards almost to the horizon, and which, for a while increased in splendour and dimensions in proportion to the number of beams assembled. This extraordinary streak of light continued in full splendour for about two minutes, when it began to wane, and its gradual decrease in both intensity and dimensions, until its final disappearance, formed the closing part of this second grand act of the meteoric drama.

We now come to the last, and by far the most magnificent spectacle of the whole. It began about ten o'clock. Its general character was similar to that last described, but its splendour and duration far exceeded it. The luminous beams, in this case, issued as before from a point in the heavens near to the tail of the *Great Bear*, at that time a considerable height above the north-eastern horizon, and formed an arched line of march—for a march it really was—above the pole star, reaching exactly to the *Pleiades* westward. The length of these auroral beams was greater than that of the last described group, and terminated both upwards and downwards, in the

manner that streamers usually terminate *upwards*. These aerial spectres seemed to form a division of grenadiers, when compared with the hosts that had preceded them, not only with respect to their magnitude, but also as regards the stateliness of their movement, which was truly solemn and majestic, and well calculated to furnish the sublimest imagery for the poet, and to store the imagination of the superstitious with the most awful portentions. From the well-known interpretations which the ancients have given to certain appearances of the Aurora Borealis, some persons have been led to think that the writer of the second book of Macabees alluded to something of this kind.

"Through all the city, for the space of forty days, there were seen horsemen running in the air in cloths of gold, and armed with lances, like a band of soldiers; and troops of horsemen in array, encountering and running against one another, with shaking of shields and multitudes of pikes, and drawing of swords, and casting of darts, and golden ornaments and harness." (Book II. chap. 5.)

Although there appeared nothing like horsemen in the Aurora I am describing, it was hardly possible to resist the idea of a very formal and well regulated march of soldiers, in single rank, being strikingly imitated in this very extraordinary display of the meteor, which concluded with a long broad streak of yellowish white light—the upper extremity of which reached nearly to the *Pleiades*, and the lower almost to the horizon, forming a brilliant tail, as it were, to that group of stars. This association of the seven stars and sloping streak of auroral light was no inapt representation of the head and tail of a comet, only that the stellar group was too dull to represent the prominent part alluded to, being completely thrown into the shade by the refulgence of the auroral light.

This streak of light continued for some minutes in nearly the same position, and gradually faded away in the same part of the heavens as that in which it was formed. It began in the same manner as the streak of light previously described—that is, on the arrival of the first beams of the group, and gradually waxed in splendour in proportion to the number accumulated at this terminal of the line, until the arrival of the last beam; and, after shining in full glory for a short time, it gradually waned until finally lost amongst the stars. With this grand spectacle the most interesting part of the Aurora terminated; but a glow of light illuminated a great portion of the northern heavens the remainder of the night, and until three or four o'clock next morning. The wind was strong from the west, and piercing cold the whole night.

Theoretical Views.

With respect to the cause of this meteor, I can form no other opinion than that it originates in a sudden change of temperature in the upper regions of the atmosphere, which gives rise to a corresponding disturbance of the electric fluid, causing extensive

movements of it amongst the attenuated air and aqueous vapour, illuminating them as it spreads in various directions, according as their different parts are prepared for its reception and diffusion. It is no unusual circumstance to observe preparations, as it were, during the evening, before daylight has disappeared, for a display of auroral beams or streamers after nightfall. These preludes consist of certain arrangements of thin streaks of nubiferous matter, floating at high altitudes, and often stretching quite across the heavens, and appearing to converge at two opposite points near the horizon, forming what some people call "Noah's Ark." These streaks or bands of vapour, when traversed by the electric fluid at the night time, become *luminous conductors*, and form streamers of the Aurora Borealis—displaying different degrees of brilliancy, in correspondence with the attenuation of the nubiferous arrangement and the quantity of electric fluid flowing through it. From this simple fact, which I have myself witnessed, and from the high probability that similar arrangements of still more attenuated aqueous vapour are frequently formed at altitudes where they are far beyond the reach of observation, until illuminated by electrical disturbances, there can appear no great degree of extravagance by supposing that most, if not all, streamers assume their peculiar forms from a like cause. It is possible, however, that on many occasions the electrical disturbances may take place even at higher altitudes, and the light be transmitted through the thinnest bands of these nubiferous arrangements, which would give the appearance of streamers, or luminous beams, as decidedly as if they were themselves the conductors or channels of electrical transmission.

The streamers, which mostly constitute a conspicuous feature in the Aurora Borealis, are not often suddenly formed; they generally spring from some definite speck in the heavens, and wax gradually to their full dimensions, and then as gradually fade away. Some streamers, it is true, shoot rapidly to their full growth, and almost as suddenly disappear; but in all cases they can be seen expanding lengthwise, whatever may be the rapidity of their growth. They sometimes lengthen in both directions, but most frequently in one direction only—a circumstance more favourable to the idea of the nubiferous bands being the media of transit than in the capacity of transparent screens, permeated by an electric light from above.

In many displays of the Aurora, floods of streamers *appear* to flow upwards, from every part of a luminous bow, which crosses the meridian in the north, and stretch to various angles of altitude towards the spectator—some of them reaching to his zenith, whilst others terminate their career before they arrive midway but; in no instance do streamers spring into existence mature or full grown. This fact also gives countenance to the idea of their consisting of streaks or bands of thin aqueous vapour, gradually, though rapidly in some cases, illuminated longitudinally by transmissions of the electric fluid. This view is still further supported by the fact that, in whatever direction streamers may be elongated, the point from which they spring is the most

intensely luminous of the whole, and becomes the *base* of the group; from this base or starting point, the light becomes more and more attenuated, until at last it softens gradually and melts into the normal light of the sky, and is lost. This gradual decay of brilliancy, during the progress of the streamers from their birth-place to their terminal points, has every appearance of a gradual dispersion, and consequent attenuation of the electric fluid, as it flows along the aqueous conductors, until eventually it becomes so enfeebled as to be incapable of displaying a sufficiency of light to be traced by the eye any further in its progress towards its destination. Hence also the different distances to which streamers reach from their respective birth-places: some fade away and are lost within a range of a few degrees, whilst others progress through an immense span in the heavens, but in all cases terminating in nearly the same manner.

Nor are these the only indications of the Aurora being within the limits of the atmosphere. The colour of the meteor is not that of an electrical light in a vacuum, nor in very highly attenuated air; but such as would be produced by floods of the electric fluid amongst attenuated aqueous vapour. The auroral light is that of a pure candle flame, and sometimes of a silvery white, neither of which can be imitated by electrical transmissions through a vacuum.

Philosophers have long been endeavouring to ascertain the height of the auroral arch, when displayed in the northern heavens, but hitherto no two of them have arrived at similar conclusions. Some have supposed it to be only a few miles above the earth's surface, and others have given it a height of above a thousand miles. From some calculations made by the late Dr. Dalton, he infers that "the height of the *rainbow-like arches of the Aurora* above the earth's surface is about one hundred and fifty English miles."*

If the Aurora be an electrical meteor, as is now generally admitted, the northern arch, which invariably assumes the white flame colour, must necessarily be situated within the limits of the atmosphere, for the reasons already stated. The streamers also, which are generally of the colour of the arch, are obviously within a similar range from the earth's surface, and from the hazy condition of the air through which they sometimes appear to progress, added to the fact that they often conform themselves to the figure and position of certain formations of cloud, there appears much reason to believe that streamers are displayed within the regions of aqueous vapour.

With respect to the apparent ascent of streamers, it is nothing more than the effect of perspective, and ought not to be understood that those parts of the meteor rise to a higher region from the earth's surface; but, in the same sense as a cloud is said to rise, or the sun, moon, or any other heavenly body is understood to rise, which implies no increase of distance between the earth and the body, but merely an increase in the

* Meteorological Observations, &c.

vertical angle, formed by the visual ray from the body and the plane of the horizon ; it is therefore the elongation of a streamer *towards* the zenith of the observer that causes the appearance of ascent or shooting upwards, and no real increase of distance from the earth's surface. When streamers pass the zenith, their increase in length gives them the appearance of a *downward* motion, which is also the case, whatever may be their course, provided the progress of elongation is *from* the spectator.

From the observed influence of electrical forces in giving forms to, and producing intestine commotions in, thunder-clouds, there is reason to infer that similar forces are productive of peculiar forms and arrangements of aqueous vapour in regions much higher than those groups of heavy clouds ; and that electrical transmissions may be accomplished through conductors which had been formed by electrical forces, but no such formations of conducting material could take place exterior to the atmosphere, where none is in existence.

It is a well-ascertained fact that the electric fluid is more abundant in the upper parts of the atmosphere than in the inferior strata ; and that this is the normal state of the air when undisturbed by clouds or other causes. Hence, were this normal state to remain unruffled, there would be a steady equilibrium of electric forces throughout the atmosphere, and an electrical tranquillity would be permanently established in every part of it. Such a tranquillity, however, cannot possibly exist in an atmosphere that is subject to continual fluctuations of temperature, moisture, and consequent winds ; hence the natural tendency to an electrical equilibrium is ever being interrupted, and electrical commotions of more or less magnitude are continually going on.

When the air is highly charged with aqueous vapour, and suffers a sudden depression of temperature, dense clouds are formed with amazing rapidity, and the electric fluid being condensed in them to a higher degree of intensity than they can retain it, liberates itself from this aqueous imprisonment in the shape of lightning. In the upper regions of the air, where the insulation is much less perfect, no lightning cloud can possibly be formed ; because the electric fluid, finding but little resistance to its movements, however suddenly it may be disturbed by change of temperature, flows from one part to another before its intensity gets sufficiently high to form lightning or discharge itself in a close compact body ; hence, any sudden disturbance of the electric fluid in the upper regions of the atmosphere, instead of producing lightning, would cause it to move in waves, or as a diffused ocean, in those directions offering the least resistance, covering an extensive area in its transit. Such an electric tide occurring at night-time would be visible, and partake of all the forms of the conducting media through which it passed.

Now, it being a well-established fact that attenuated air is a better conductor than air of greater density, and that a vacuum is a better conductor than attenuated air—and, as the attenuated regions of the atmosphere are more highly charged with the

electric fluid than the dense air below, analogy would lead to the inference that the electric fluid is still more abundant exterior to the shell of air than anywhere within it, an inference which will readily be conceded by those who allow that the Aurora Borealis is an electrical phenomenon displayed at elevations far beyond the reach of the atmosphere. But here it is that an insuperable difficulty presents itself in finding the disturbing agent. Within the atmosphere, electrical disturbances are easily accounted for by the influence of well-known agents ; but, at the distance that some philosophers have placed the Aurora from the earth, such agents are not known to exist.

The electrical theory of the Aurora Borealis, as it exists at the present day, is considerably alloyed with the Magnetism of the earth. Halley appears to be the first on the list of those philosophers who have called in terrestrial Magnetism to assist in explaining the cause of the Aurora Borealis; but the circulating *magnetic effluvia* of this eminent philosopher appearing insufficient for the views of Dalton, the latter invented " an elastic fluid partaking of the properties of *iron*, or rather of *magnetic steel*," which he placed in the upper regions of the atmosphere, in " the form of cylindrical beams," which, when illuminated by the electric fluid, become the beams or streamers of the Aurora Borealis ; and " the rainbow-like arches," says this philosopher, " are a sort of rings of the same fluid, which encompass the earth's northern magnetic pole, like as the parallels of latitude do the other poles."*

The frequent position of the auroral arch with respect to the magnetic meridian, and the occasional disturbance of the magnetic needle during an auroral display, are well calculated to associate terrestrial Magnetism with the theory of the meteor ; but it would be difficult to imagine how the extravagant appendages of Dalton could be required to give " an irregular oscillation to the horizontal needle," which amounted to no more than half a degree on each side of its " mean daily position ;"† especially as the principles of Electro-Magnetism were as well known at the time the last edition of the hypothesis was published (1836), as they are at the present day ; and would as easily have accounted for the needle's movements independently of those appendages as with them. And although other observers have met with much greater movements of the magnetic needle during an Aurora, I can see no reason for supposing that they were due to ferruginous matter floating in the atmosphere, because the well-known principles of Electro-Magnetism are, independently of any such ferruginous elements, quite sufficient to accomplish their production.

The hypothesis of Dalton, with some slight modifications, being that in most repute at the present day, and favoured by the views of some of the most illustrious philosophers, require more than an ordinary consideration ; and fortunately being expressed in terms that cannot well be misunderstood, it may be examined without any apprehensions of mistaking the principles on which it is founded.

* Meteorological Essays, page 169. † Ibid. page 171.

In order that our author might not be obscured in his views, he particularly states the difference between the *magnetic effluvia* of Halley, and the *ferruginous matter* of which he constructs his cylindrical magnetic beams. "It may perhaps be necessary here, before the subject is dismissed," says Dalton, "to caution my readers not to form an idea that the *elastic fluid of magnetic matter*, which I have all along conceived to exist in the higher regions of the atmosphere, is the same thing as the *magnetic fluid or effluvia* of most writers on Magnetism. This last they consider as the efficient cause of all the magnetic phenomena; but it is a mere hypothesis, and the existence of the *effluvia* has never been proved. My *fluid of magnetic matter* is, like magnetic steel, a substance possessed of the properties of Magnetism, or, if these writers please, a substance capable of being acted upon by the magnetic effluvia, and not the magnetic effluvia themselves."

It is somewhat remarkable that, after such an abrupt dismissal of all preceding attempts at explanation, the hypothesis of Dalton should appear the most extravagant that has hitherto appeared in the history of the Aurora Borealis. We have no knowledge whatever of the existence of this imaginary ferruginous effluvium; nor would any Magnetist ever suppose that such a fluid, even were it admitted to have an existence, would put on the form of *cylindrical beams*, and at the same time adapt itself into *rings* round the magnetic poles of the earth. Moreover, as this imaginary fluid is supposed to be floating within the atmosphere, the hypothesis is left in a state of imperfection from a want of information respecting the author's mode of expanding the shell of air to the thickness of 150 miles, the height at which he has placed the Aurora Borealis.

The same hypothesis supposes that the auroral beams are "similar and equal in their real dimensions to one another"—an assertion by no means sanctioned by observation; but, on the contrary, perfectly at variance with the appearances generally. The hypothesis also supposes that the auroral beams are all "parallel to the *dipping needle* at the places over which they appear;" and that "the point in the heavens to which the *beams* of the Aurora appear to converge at any place is the same as that to which the south poles of the dipping needle points at that place." With all due respect for the philosophical ability and skill of Dr. Dalton, the cause of science has a predominating claim to our regards over all other considerations in discussions of this nature: there can, therefore, be no impropriety in stating that, were there no other observations to discountenance this part of the hypothesis, those on the Aurora of the 17th November last would be sufficient to prove its inaccuracy.

That the auroral arches, when they appear in the north of these latitudes, cross the magnetic meridian at nearly right angles, is a fact very frequently observed, though it is by no means its universal position. The highest point of the arch is probably as frequently in other positions as in the magnetic north: it is sometimes several degrees eastward of the true north, at other times due north, and on many occasions it never

appears at all. To admit that the arch is a visible part of a complete ring that surrounds the magnetic pole of the earth, and that at the same time it crosses at right angles the magnetic meridian of every place of observation, would be to admit a complete system of confusion—in fact, an absurdity. According to Hansteen's and Barlow's maps, the curve of *equal variation* that passes through Great Britain passes also a little north of the Western Isles, through Newfoundland and into Hudson's Bay; and, in the other direction, it passes through the North Sea, the Shetland Islands, and thence almost direct north past Spitzbergen. No *circular ring* that could possibly be imagined to surround the north-western magnetic pole of the earth, would answer the other parts of the hypothesis, for all the magnetic meridians of that particular *curve of equal variation.* In Hudson's Bay the magnetic meridian would be at right angles to the magnetic meridian of Great Britain; and in many parts of the curve there would be such obliquities of the magnetic meridians to each other, that but very few of them would cross tangents to the supposed ring, at right angles, and at the point of contact—circumstances required by the hypothesis; or, in other words, there are but very few magnetic meridians in this curve of equal variation that run sufficiently close to the north-western magnetic pole of the earth to satisfy the conditions of the hypothesis. In Great Britain, and throughout the northern parts of the curve, the magnetic meridians pass north of the pole; and the magnetic meridian, for the same curve, opposite the coast of Spitzbergen, would pass the magnetic pole northward upwards of 10° of latitude. Westward, over the Atlantic Ocean, the magnetic meridians of this curve would approach the north magnetic pole more closely; and, in Newfoundland, the magnetic meridians would probably pass through that pole; but on the American Continent, towards Hudson's Bay, the magnetic meridians would pass the north-western pole some three or four degrees on its south side.

I have selected this particular curve of equal variation, as it stands in Professor Barlow's map, because it is that which passes through the North of England, and corresponds with the variation at Kendal (25°) when Dr. Dalton made his observations. Had the selection been made on the curve of 20°, which passes through the most western parts of Spitzbergen, through Norway, France, Algiers, and the Canary Islands, thence across the Atlantic to Nova Scotia, Canada, and Hudson's Bay, the deviations of the magnetic meridians from the magnetic pole would have been much greater, especially in Europe, where the meridians have been more exactly ascertained than in any other part of the curve—that is, through the whole of the curve from the Mediterranean to Spitzbergen; in which the magnetic meridians would cross the meridian in which the north-western magnetic pole is situated on its northern side, and the magnetic meridian of Spitzbergen would cross the meridian of the pole 15° or more north of it. In the western portion of the curve, however, from Algiers to Hudson's Bay, the magnetic meridians would run sufficiently close upon

the magnetic pole to answer the conditions of the hypothesis. But the deviation of the *western line of no variation*, from the meridian of the magnetic pole, would alone be sufficient evidence of the incorrectness of the hypothesis.

In all these cases, the north-western magnetic pole is supposed to be situated in 69° 53′ north latitude, and 93° 33′ west longitude, as calculated for the year 1800, which is the nearest date on record to 1793, the year in which Dr. Dalton first published his theoretical views of the Aurora Borealis, and which are those that still appear in his last edition, published in 1836.

In referring again to that part of the hypothesis which places the auroral beams "parallel to each other," and at the same time "parallel to the *dipping needle* at the places over which they appear," it is obvious that, to fulfil these conditions, the dipping needle would have to assume one and the same position, both in dip and direction, at all places over which the auroral beams appear at any one time—that is, the dipping needle would have to be parallel to one individual right line, at all places of observation, however wide apart. Now, without taking into consideration the difference of dip at different places over which auroral beams often appear at the same time, the different positions of the magnetic meridians, in the planes of which the axis of the dipping needle would repose at those places, would be quite sufficient to show the fallacy of that part of the hypothesis.

It is somewhat remarkable that neither Dalton nor any other philosopher that I am aware of, has taken into consideration the electro-magnetic forces of auroral beams or streamers in disturbing the magnetic needle. These forces are brought into play by every movement of the electric fluid, whether ferruginous or other metallic matter be present or not, as well in the most perfect vacuum as in dense air; and when such immense floods of the electric fluid are put into motion as constitute a grand Aurora Borealis, the principal features of which are extensive groups of streamers, it is to be expected that the electro-magnetic forces of those streamers will disturb the compass needle; causing deflections of different degrees of magnitude, and in different directions, in correspondence with the intensity and direction of the disturbing forces: and, all other things being the same, the greatest deflections of the horizontal needle would be accomplished by electric streamers that were parallel to it, and consequently parallel to the earth's surface at the place of observation. The supposition of the auroral beams being vertical or nearly so, and at remote regions above the atmosphere, may possibly have been the cause of the electro-magnetic forces of streamers being so generally overlooked; but it has long appeared to me that to their influence the observed disturbances of the needle are principally if not solely owing.

Although the theoretical views which I have taken dispenses entirely with the ferruginous effluvium supposed to be floating in the air, I by no means attempt to

deny its absolute existence, nor the existence of other metallic effiuvia. My motive for contending against the influence of such an agent in producing auroral beams is to show that it is quite unnecessary for the purpose it was intended, and the manner in which it has been applied preposterous. The manner in which I have attempted to explain the several phenomena attending the Aurora Borealis requires no other elements nor forces than those well known and understood. The same cause—a sudden depression of temperature—that produces lightning amongst the clouds, would produce an Aurora Borealis in a higher region of the air. A depression of temperature at the earth's surface invariably succeeds a lightning storm, and almost as certainly closely follows an Aurora Borealis; and very often both of these electrical phenomena appear at the same time—showing that the disturbance extends to a great height in the atmosphere; and the fall of temperature below that succeeds these phenomena, as well as the immense hail-storms that attend lightning, infer that the change of temperature commences far above the earth's surface, and that it progresses *downwards* with various degrees of speed.

The observations which I have myself made on the predisposition of clouds for a display of the Aurora Borealis are similar to the observations of Captain Back, at Fort Reliance (north latitude 62° 46′, and west longitude 109°). "The Aurora was frequently seen at twilight, and as often to the eastward as the westward. Clouds also were often perceived in the day time, in form and disposition very much resembling the Aurora."

The same scientific officer observed also, that "dense fog, in conjunction with an active Aurora, was uniformly favourable to the disturbance of the needle;" and, "when seen through a hazy atmosphere, and exhibiting the prismatic colours, almost invariably affected the needle." These observations are of great interest, both as regards the decomposition of the electric light and the production of magnetic disturbance—showing, in my opinion, that the fog was essential in the production of colours, as well as to the transit of a portion of the electrical fluid, at no great distance from the needle; and this view is strongly supported by the opposite effects of the Aurora, when no fog or haze was present. "On the contrary," says Captain Back, "a very bright Aurora, though attended by motion, and even tinged with a dullish red yellow, in a clear blue sky, seldom produced any sensible change of the needle beyond, at most, a tremulous motion."*

There is a great difference in the character of auroral displays, scarcely any two being alike: some of them appear to be of such a complex and mysterious character

* Mr. Dancer, optician, of Manchester, whilst observing the white light of one of the Auroræ described in this paper, breathed upon a pane of glass through which he was looking, and immediately the prismatic colours appeared. The same effect is produced to passengers travelling in a close coach, and looking at the gas-lights through a window covered with aqueous particles from breathing; a beautiful prismatic iris is seen around the burning gas.

as to bid defiance to scientific investigation, whilst others develop a peculiarity of features that can hardly be misunderstood, and may with propriety be considered as keys of admission to the whole. Amongst the latter may be enumerated those in which are observed a predisposition of nubiferous matter during daylight—the luminous streaks or bands of vapour—the waves of light that shine across the eye of the spectator—the hazy character of the atmosphere—and also the transcolourations of the light—all of which have appeared in unusual abundance during the past season, or since the commencement of last autumn.

Dr. Halley gives a very precise account of the appearance of luminous vapour, haze, and streaks of light, in the Aurora of the 16th March, 1716. This eminent philosopher tells us that he did not see the Aurora till about nine o'clock; but, at that time, he "immediately perceived, towards the south and south-west quarter, that, though the sky was clear, yet it was tinged with a strange sort of light; so that the smaller stars were scarce to be seen, and much as it is when the moon of four days old appears after twilight. I perceived, at the same time, a very thin vapour to pass before us, which arose from the precise east of the horizon, ascending obliquely, so as to leave the zenith about 15° or 20° to the northward; but the swiftness wherewith it proceeded was scarce to be believed, seeming not inferior to that of lightning, and exhibiting, as it passed on, a sort of momentaneous *nubecula*, which discovered itself by a diluted and faint whiteness; and was no sooner formed but, before the eye could well take it, it was gone, and left no signs behind it. Nor was this a single appearance; but for several minutes, about six or seven times in a minute, the same was again and again repeated—these *waves of vapour* regularly succeeding one another, and at intervals nearly equal, all of them in their ascent producing a like transient *nubecula*. By this particular we were at first assured that the *vapour* we saw became conspicuous by its own proper light."

In this noted Aurora there was no light seen in the north till about eleven o'clock. "On the western side of the northern horizon—viz. betweeen west and north-west—not much past ten o'clock, I observed," says our author, "the representation of a very bright twilight, contiguous to the horizon, out of which rose very long beams of light, not exactly erect towards the *vertex*, but something declining towards the south; which, ascending by a quick and undulating motion to a considerable height, vanished in a little time, whilst others, at certain intervals, supplied their place; but, at the same time, through all the rest of the northern horizon—viz. from the north-west to the true east—there did not appear any sign of light to arise from or join to the horizon, but what appeared to be an exceedingly black cloud seemed to hang over all that part of it; yet it was no cloud, but only the serene sky, more than ordinary pure and limpid, so that the bright stars shone clearly in it."

The Doctor next mentions "two *laminæ* or streaks" of light, "lying in a position from the north by east to the north-east, and were each about a degree broad—the undermost about eight or nine degrees high, and the other about four or five degrees over it: these kept their places for a long time, and made the sky so light, that I believe a man might easily have read an ordinary print by the help thereof." And again—"It being now past eleven of the clock, and nothing new offering itself to our view but repeated *phases* of the same spectacle, I observed that the two *laminæ* or streaks parallel to the horizon had now wholly disappeared; and the whole spectacle reduced itself to the resemblance of a very bright *crepusculum*, setting on the northern horizon, so as to be brightest and highest under the pole-star itself, from whence it spread both ways into the north-east and north-west."

About the time that this Aurora appeared, the variation at London, the place of observation, was about 18° westward; and consequently neither the two streaks of light, nor the *crepusculum* in the north had any relation to the magnetic meridian. The *nubecula* seen by Dr. Halley seems to have been of the same character as the flashes or waves of luminous vapour, seen at Prestwich during the Auroræ of November 21st and December 17th last: they were obviously of no very great altitude—certainly within the range of aqueous vapour. The colour of these waves was that of a dim silvery whiteness.

I perfectly agree with Halley, Hansteen, Brewster, and many other eminent philosophers, in the belief of a magnetic element or effluvium pervading the atmosphere, and perhaps all space; but the principles of Electro-Magnetism do not allow of electric currents traversing the magnetic lines of force in the direction of their length, unless constrained by other influences than any known to exist in the regions of the Aurora Borealis. It is possible, however, that the theoretical views which I have here advanced may be open to objections that I do not myself perceive, and may require the corrections of a more diligent observer, and a sounder reasoner on the facts observed. W. S.

Prestwich, 1849.

SECTION V.

OBSERVATIONS ON THUNDER STORMS, ELECTRICAL KITE EXPERIMENTS, &c. BY WILLIAM STURGEON.

*Description of a Thunder Storm, as observed at Woolwich; with some Observations relative to the Cause of the Deflection of Electric Clouds by High Lands; and an Account of the Phenomena exhibited by means of a Kite elevated during the Storm.**

On Saturday evening, June 14th, about eight o'clock, an electric storm passed partly over this place, exhibiting lightning the most splendid I ever beheld. The wind was pretty brisk, from south to west, about the first appearance of the storm; and if the electric clouds had obeyed the force of the wind only, the principal part of them would have come directly over us. This, however, was not the case; for, instead of their being carried over Woolwich in the direction of the wind, the most formidable group of them, and consequently the greatest fury of the storm, were deflected out of the wind's track before their arrival at Shooter's Hill; and were carried over the low lands on the other side of the hill, towards the Thames, in a direction nearly from west south-west to east north-east.

The deflection of electrized clouds out of the wind's direction, though perhaps not much noticed, is a very common circumstance in the neighbourhood of high lands, especially if those lands are composed of materials which are bad conductors of Electricity; for, although they do not absolutely refuse the transmission of the electric matter driven to the surface of the hill from the lower strata of air,† by the disturbing

* From the London and Edinburgh Philosophical Magazine, for December, 1834.

† The asperifolious plants, and the vegetable clothing of the land generally, especially at this season of the year, receive the electric fluid from the atmosphere in great abundance; and the myriads of vegetable points, sharp edges, &c. presented to the air, offer every facility for its reception on any emergency of pressure emanating from the repulsive force of a highly charged cloud. The surface of the land thus becomes charged at the expense of the air, each gradually resuming its natural electric equilibrium again, as the disturbing force withdraws its influence, by the progress of the cloud in its course. Thus new tracts of country become charged in succession as the cloud approaches them, and an electrical tide sweeps the face of the land by the floating influence above.

force of the condensed electric fluid in the clouds, the transfusion into the ground is too tardily performed to prevent accumulation on the surface, which consequently becomes charged in the same state as the clouds that are approaching it. A reaction immediately takes place, and a consequent repulsion or deflection of the clouds is produced. This electric force, now operating in conjunction with the wind, gives the cloud a new direction of motion, and urges it over a tract of country composed of better conductors, which are more susceptible of being transpierced by the electric matter than those from which the cloud was deflected. Hence it is that electric storms are more frequent and more violent over marshy lands, rivers, &c. than over drier and more elevated tracts of country.*

These causes operated in a very beautiful manner in giving direction to the storm on Saturday evening. The principal group of clouds, as before stated, never reached Shooter's Hill, but was carried over the low wet lands on the other side to the Thames; and the foremost clouds were taken to the other side of the river, and over a considerable tract of the Essex marshes. At this period, another direction was given to the storm, and the new combination of forces urged it in the direction of the river, a route which electric storms visiting this neighbourhood very frequently take.

Its progress down the river was exceedingly slow, owing, as I suppose, to the wind (though much slackened before this time) being more directly opposed to it. I had been floating an electric kite in the Artillery Barrack-Field during the transit of that

It is on this account that insulated kite-strings, exploring rods, &c. frequently become negatively electric at the approach, and during the transit, of clouds of this description. But if the kite or the exploring rod were to reach into the cloud, it is not likely that either of them would ever be found in a negative state. I am speaking of the principal influencing cloud, and not of those straggling thin patches in their vicinity, which frequently become negative by a portion of the electric matter which they before possessed being driven out of them by the predominating electric force of the superior cloud. In the same manner an insulated metallic rod, furnished with fine points or sharp edges at its further extremity, may have its natural electric fluid driven out of it into the air, by the approximation of a positively charged body at the other end.

I have never yet found the atmosphere *negative* with regard to the earth at any other time than when modified by such causes as I have pointed out. I have made upwards of four hundred electric kite experiments, under almost every circumstance of weather, at various times of the day and night, and in every season of the year; I have experimented on Shooter's Hill, and on the low lands on the Woolwich and Welling sides of it, and the experiments in the three different places within an hour of each other; I have done the same on Chatham lines, and in the valley on the Chatham side of them; on Norwood Hill, and in the plain at Addiscombe; also on the top of the Monument in London; and, during the present year, on the top of some of the high hills in Westmoreland and in the North Riding of Yorkshire, and in every case I have found the atmosphere positive with regard to the ground. In most of these cases, the stations at the tops of the hills were higher than the place of the kite when the experiments were made at the lower ones.

I have floated three kites at the same time, at very different altitudes, and have uniformly found the highest to be positive to the other two, and the centre kite positive to that which was below it: consequently the lowest one was negative to the two above it, but still it was positive to the ground on which I was standing. I have made more than twenty experiments of this kind, and the results (with the exception of electric tension) were invariably the same; showing most decidedly that the atmosphere in its undisturbed electric state is more abundantly charged than the earth, and, as far as I have been able to explore it, still more abundantly in the upper than in the lower strata.

* Immense tracts of flat country frequently become charged in the same manner and from the same cause as ranges of hills are charged; but the repulsive force from such places is directed vertically, and not so directly opposed to the horizontal direction of the cloud's motion as that proceeding from the side of a high hill.

part of the storm which passed over Woolwich. I had got completely wet with the heavy rain which fell during the time; notwithstanding which, the unusual fineness of lightning which was playing over the river and marshes induced me to pursue it with my eye, when, from its distance, I could no longer explore the theatre of its resplendent exhibition. I walked to the top of Wellington-street, from which place I had an exceedingly fine view of the storm, now too distant to hear even a feeble murmur of those thunders, which, I am persuaded, were, to the inhabitants immediately in their vicinity, terrible to an unusual degree.

It was now nine o'clock, and the lightning was magnificent indeed. Nature appeared as if disposed to gratify the utmost extent of curiosity by an unremitting display of her electrical elemental fire.

For about half-an-hour, the storm appeared to be nearly stationary, hovering over a tract of low land on the Essex and Kent sides of the Thames—perhaps not far from Purfleet. The lightning was unusually refulgent, the flashes in rapid succession, and discharged in every possible direction that can be imagined, and generally through a longer striking distance than I had ever before noticed. Three or four discharges, which occurred a little after nine o'clock, darted through a horizontal arch of about 50° each; and several of these which were directed vertically, and oblique to the horizon, shot through 30° or 40°, the fluid being visible in every part of the circuit.

If this lightning was discharged over Purfleet, or thereabouts, as I have supposed, it would be about eight miles from where I was standing. Now, allowing seven miles to be the mean distance of the lightning discharged in a track at right angles to the line of sight, the angle of 50° would give a chord of nearly six miles and a half for the striking distance, or the tract of air through which the lightning travelled visibly at one discharge. The apparently vertical and oblique discharges were much nearer in some part of the circuits than those which shot through the extraordinary horizontal ranges. The rain, I imagine, was falling in torrents, which would greatly facilitate the transmission through long striking distances. Moreover, the inferior density of the air in the regions of the clouds, and the thin aqueous vapours which are floating there, tend very much to facilitate the transmission.

Buildings, trees, and other tall objects, are not usually struck by lightning before the falling of rain, the dry dense air offering too great a resistance to be transpierced from the clouds to the ground.

From the time that the clouds arrived within the influence of the Thames, they seemed to travel nearly in its direction; for although the lightning played over some miles of country on both sides of its banks, the river appeared to be the direction line to the focus of the storm, which, if not earlier disposed of, would probably be transported by it to the Nore or to the Channel—a direction nearly at right angles to that of the wind at this place.

The lightning was very fine from the straggling clouds which passed over our heads, and from others which crossed the Thames much nearer to London. These clouds separated from the splendid group already noticed, and travelled in the direction of the wind towards the north or north by east; and would, if not obstructed, discharge their lightnings as a distinct storm over the country about Waltham, Epping, Chipping Ongar, Harlow, &c.

The wind had abated to such a degree before I arrived in the Barrack-field, and the rain fell so heavily during the time I was there, that it was with some difficulty that I got the kite afloat; and when up, its greatest altitude, I imagine, did not exceed fifty yards. The silken cord also, which had been intended for the insulator, soon became so completely wet that it was no insulator at all. Notwithsanding all these impediments being in the way, I was much gratified with the display of the electric matter issuing from the end of the string to a wire, one end of which was laid on the ground, and the other attached to the silk at about four inches distance from the reel of the kite string. An uninterupted play of the fluid was seen over the four inches of wet silken cord, not in sparks, but in a bundle of quivering purple ramifications, producing a noise similar to that produced by springing a watchman's rattle. Very large sparks, however, were frequently seen between the lower end of the wire (which rested on the grass) and the ground; and several parts of the string towards the kite, where the wire was broken, were occasionally beautifully illuminated. The noise from the string in the air was like to the hissing of an immense flock of geese, with an occasional rattling or scraping sort of noise.

Two non-commissioned officers of the Royal Artillery were standing by me the whole of the time, who, unaware of the consequence, would very gladly have approached close to the string; and it was not until I had convinced them of the danger of touching, or even coming near to it, at a time when the lightning was playing about us in every direction, that I could dissuade them from gratifying their curiosity too far—probably at the expense of their lives. We anxiously and steadfastly watched what was going on at the end of the string, and the display was beautiful beyond description. The reel was occasionally enveloped in a blaze of purple arborized electrical fire, whose numberless branches ramified over the silken cord, and through the air to the blades of grass, which also became luminous, on their points and edges, over a surface of some yards in circumference. We also saw a complete globe of fire pass over the silken cord between the wire and reel of the kite-string. The soldiers thought it about the size of a musket-ball. It was exceedingly brilliant, and was the only one that we noticed.

I had no electrometer with me, nor any of the apparatus with which I perform chemical and magnetical experiments by atmospheric Electricity; hence the whole of the time on this occasion was devoted to mere observation.

The following is a notice, extracted from the *Lancaster Gazette* of the 5th of April, 1834, of some experiments made with an electrical kite, at Kirby Lonsdale, in Westmoreland:—

On Saturday, the 29th of March, I had a very favourable opportunity of demonstrating experimentally to several of my friends at Kirby Lonsdale, who had attended my recent course of lectures at that place, that an abundance of the electric fluid usually attends hail and snow storms. The wind was pretty brisk, cold, and from the west by north nearly the whole of the day. There were several hail-showers, each of which, with a simultaneous increase of wind, became a complete transient storm.

During three of these hail-storms, I floated one of my silken electric kites, with a wired string of about 300 yards long, and insulated in the usual way by means of a silken cord.

The kite was elevated in each experiment about ten minutes prior to the arrival of the hail-storm, and the electric state of the atmosphere ascertained, which was found to be so exceedingly feeble that not the slightest spark could be observed. As, however, the cloud from which the hail was falling approached the kite, the fluid from the string presented itself in brilliant sparks to the knuckle; and, during the transit of the cloud, became so abundantly discharged to a wire presented to the string, that it struck in rapid succession through a stratum of six inches of air; and through three inches of air, it presented a splendid continuous stream of electric fire. As the cloud receded from the kite, by advancing in its aerial course, the electric discharges became less and less brilliant, and continued to diminish in splendour and energy with the recession of the passing storm, ultimately vanishing altogether by the emergence of the kite from the electric influence of the cloud.

These appearances were exhibited in each experiment, but the display of the electric fire was the most magnificent in the second, which was during the fiercest hail-storm of that day, and happened between two and three in the afternoon. During an early part of this storm, the electric fluid made a continuous rattling noise down the kite-string (in consequence of the wire being broken in several places), and darted from the reel at the inferior extremity to greater distances than in either of the other experiments. In one instance, it struck over a stick more than a yard long, to the hand of a young man named Croft, who was presenting it to the kite-string. Although the remote end of the stick was in connection with the ground by means of a very wet string—and consequently a considerable discharge must, at the same time, have passed down the wet string to the earth—the shock was so violent as to make Mr. Croft reel and nearly fall; and I have some reason to suppose that it has left an impression on his memory which time will not speedily obliterate. The kite string, however, broke very soon afterward, and consequently the experiments, on that occasion, terminated very abruptly, and unfortunately at a time also when the

fire was streaming from the string in the greatest abundance, and with a degree of splendour better imagined than described.

During the third time that the kite was afloat, about two hours after the former, several gentlemen present experienced smart electric shocks direct from the kite-string.

W. S.

Artillery Place, Woolwich, June 16th, 1834.

During a thunder storm which visited Liverpool early in the morning of Tuesday, August 24th, 1841, the lightning struck and damaged two churches, St. Michael's in Pitt-street, and St. Martin's-in-the-Fields. Both edifices are supposed to have been struck at nearly the same time—about two o'clock.

As soon as I heard that the scaffolding for taking down the damaged steeple of St. Michael's was completed, I proceeded to Liverpool to examine the edifice ; and, by the assistance of the workmen, I was fortunate enough to arrive at the top of the spire.

During our ascent my attention was frequently directed to the havock which the lightning had made on various parts of the interior of the masonry, at which, at that time, I merely took a cursory view, with the intention of examining the particulars more closely as I returned.

On reaching the top, I first examined the metallic cross and the ball in which the lower end of the shank of it was fixed, and I very shortly discovered, by the discolouration of the metal, which is bronze, that the electric fluid had first struck the lower end of the shank of the cross, and that it had not touched the upper part of it at all. The ball or cap, and cross together, are nine feet six inches high, and I estimate the cap, which is a hollow ball of bronze, to be about two feet and a half diameter, which would leave seven feet for the height of the cross above the surface of the cap. A more decisive indication of the precise spot where the electric fluid first struck the cross could not possibly have taken place than by the well-defined boundary of discolouration of the bronze. And what was more satisfactory still, the *obliquity* of this upper boundary line, or margin of discolouration, showed, in the most decisive manner, that the cloud from which the lightning proceeded was, at the time of the discharge, on the north-west side of the church ; for on that side of the shank of the cross was the highest point of the margin of discolouration, and on the opposite side was the lowest point of that margin ; and the figure of this upper margin of discolouration was something of an ellipse placed round the shank of the cross, sloping downwards from the north-west to the south-east side.

The highest part of this boundary or margin is not a foot and a half above the surface of the cap, leaving above five feet of the best conducting metal above that point untouched by the lightning. The gilt bronze from that margin down to the horizon-

tal equator of the cap is converted into a leaden blue colour. At the horizontal equator of the cap, the discolouration becomes scattered and lost in a multitude of ramifications on the lower half of the ball. The ramifications are precisely similar to those made on the surface of a glass jar by spontaneous discharges. This is one of the most striking evidences that has come to my knowledge of an *oblique* discharge of lightning striking a good conductor at so great a distance from the highest point.

From the cap descends a stout cylindrical metallic bar, which I was told is either of copper or bronze (gun metal), but which more probably is iron. The spire, with the exception of a few feet at the top, is hollow, and the bar proceeds downwards nearly forty feet, and rests in the intersection of two stout flat iron bars, crossing one another at right angles; hence from the shank of the cross, where the lightning first struck, to the bottom of the central rod, no damage was done to the spire. But, as the lightning no longer found a continuous conductor, it commenced its destructive effects on the masonry at the extremities of the cross iron bars on which the central bar rested. I cannot say that the effects produced were different from what might have been expected from a heavy flash of lightning striking an edifice whose materials were similar, and similarly distributed, to those which constituted this steeple. The stones are held together by copper clamps, and the spare lead which had been left in the ladles, after soldering the clamps into the stone, seems, whilst yet in fusion, to have been carelessly poured into the crevices between the stones; forming, when cooled, solid bars of lead of considerable dimensions, both in length and breadth. Several of these masses of lead were completely dislodged by the electric force, and many scores of pounds might have been easily removed by the hand only from the rents made in the spire. I brought a few pieces with me, which are now lying on one of the tables of the gallery of this Institution.

The effect, generally below the iron rod, would seem to have been accomplished by a formidable expansive force in the interior of the spire, which has operated in two diametrically opposite sides to the greatest extent; or, at least, the effects produced are greatest diametrically opposite each other in all those places where absolute rents are made in the masonry; and the rents themselves are such as to indicate an expansive force from within. In several places, however, large masses of masonry are split off the inside of the spire, but by far the greatest quantity of shattered stone is carried outwards. There are some vertical rents at the octagonal angles more than twenty feet long; and in all cases where there is a rent on one side, there is another on the opposite side of the steeple.

The axle of the clock bell is broken, and the bell itself leaning on the loft floor, probably in consequence of a large mass of displaced masonry falling upon it; but there can be no doubt whatever of the lightning making a stepping-stone of this bell to facilitate its transit from the last place of damage above it to the metallic works of

the clock below. From the works of the clock the lightning has darted through the wall close to the lower edge of that face of the clock, which is over the body of the church, and has peeled a considerable slice off the outer side of the stone work. This appears to have been the last point of destruction, from which it no doubt darted to the lead work of the roof of the body of the church, and thence to the ground by the conduction of the metallic water pipes.

In conclusion I may add, that had the architect been disposed to build an edifice for the precise purpose of being destroyed by lightning, he could scarcely have improved upon the plan exhibited by this spire, either as to the choice or the distribution of the materials.

Remarks.—Now as it appears that these elegant tall thin spires cannot be held together against the force of heavy winds independently of metallic clamping, and as we have now, I hope, sufficient evidence of the danger of this plan, should lightning happen to strike the upper part of the edifice; and also, as great facilities for such events are afforded by the distribution of metal amongst the masonry by such mode of protection against less formidable elements, it behoves those who, in future, may have the planning and erecting of tall spires to provide especially against the effects of lightning.

Now it appears to me that had the cloud which discharged the lightning against St. Michael's Church been at a less angle of elevation, the cross would not have been hit at all, but that the lightning would more probably have struck some part of the central metallic rod below; under which circumstances the masonry must have been penetrated before the rod was arrived at, for lightning regards not stone walls when they happen to be in its way.

From these considerations it would appear that a lightning rod passing through the interior of a tall spire, although it were exposed at the top, and also in good connection with the ground at the lower end, would not give complete security to the building, especially against oblique discharges, when the angle of descent was very low. A single exterior rod would be preferable, because if the lightning happened to come from a cloud on the same side of the edifice, the rod would be struck before the lightning arrived at the masonry; but a single exterior rod might possibly be the means of much damage from lightning which approached from the opposite side. Therefore, if we are disposed to give a decided protection to such edifices from the destructive forces of lightning, *three rods* at least, at equal distances from one another, ought to be exteriorly arranged from the top to the bottom of the spire, and continued down to the ground. Such an arrangement of cylindrical copper rods of moderate dimensions would operate in concert by being united at the top, and metallically connected at two or more places below, and within fifty feet from the apex of the spire. Such a system of conductors might be so disposed as to become

even an ornament to the spire ; and for protection against lightning I am not aware of a better. Should the spire be surmounted by a cross or any other metallic ornament, the system of lightning conductors ought of course to be united with it. Metallic points and sharp edges have great influence, and must always have a superiority over blunt or rounded terminations at the upper extremity of any system of conductors whatever, in preparing the way for a flash of lightning in their own direction, on which account they ought to be strictly avoided.

W. S.

Royal Victoria Gallery of Practical Science,
Manchester.

"The storm," says the *Liverpool Standard*, "as experienced by those on board ship in the river, was at once sublime and terrific. Lieutenant Wilson, commander of H. M. S. *Etna*, lying in the Sloyne, describes the storm in a most interesting manner. He states, that the first flash of lightning appeared to strike the Mersey opposite the town, and the *Etna* vibrated from the mast-head to the keel, so that all on board conceived for a moment that she was shaken to pieces.* The parties on deck (including Captain Wilson, who kept his own watch) lost their sight for several seconds, but on recovering found that no damage was done to the ship. The second flash appeared to evolve from the southern extremity of a dark cloud, and flashed from one extremity of the town to the other."

The electric storm of the 14th September, 1841, traversed Cumberland, Westmoreland, Durham, Yorkshire, Lancashire, Cheshire, and Derbyshire, at all of which places the lightning and thunder were most terrific ; and it is highly probable that it visited many other parts of England. In Yorkshire and Lancashire the night was splendidly illuminated by the rapid succession of lightning discharges.

At Lymm, in Cheshire, about eleven o'clock at night, the lightning fell in a garden belonging to J. Leaf, Esq., and the crash of thunder that attended it terrified the whole neighbourhood for several miles round. The noise was tremendous even six miles distant. Thinking that considerable damage must have been done to some part of the premises by the effect of such a formidable discharge of lightning, Mr. Leaf and some of his family were induced to go out and make the necessary examination. The buildings, however, had sustained no damage, but in the garden, in the rear of the house, a large hole had been broken in to the depth of a yard and a half. About a week after this occurred, I was told of the circumstance by the Rev. Mr.

* This is one of those interesting facts which are worthy of particular notice, and shows that a vessel being *shaken* during a thunder storm is no evidence of its being struck by lightning. (See report from the *Beagle*, page 358.)

Johnson, who thought it possible that some drain might have been the destination of the lightning, and that it had broken into it, and the soil fallen down as a matter of course ; or it was possible that a meteorite might have fallen. These opinions appearing to be well founded, and the parties wishing me to visit the spot, I repaired thither, about fifteen miles from Manchester, as soon as I could make it convenient. The hole was partly in a pea-bed and partly in a potato-bed, its centre being directly between them.

I first got Mr. Leaf's servant to clear away the sunken soil from the bottom of the hole; which done, and deepened to nearly two yards and a half, he found a nearly horizontal cavity in the substratum of sand. The hole sloped a little downwards, and was large enough for the man to creep into, but it proceeded only about a yard and a half in that direction. The Rev. Mr. Hutchinson, who is next door neighbour to Mr. Leaf, sent his servant man to our assistance, and by the joint labours of the two men, a great quantity of earth was soon cleared away; and although no meteorite was found, there were sufficient indications of a formidable power being at work. Broken pea-sticks, pea-straw, split potatoes, and soil from the surface, were found at various depths as low down as five yards; and in several places the soil which was forced down, which is quite mellow on the surface, was compressed into a state of compactness equal to that of stiff brick clay. Several small stones also, such as whin-stones, being found split into fragments, and exhibiting recent fractures, and these at different depths indicated that a violent force had been exerted. The probability is that a discharge of lightning had struck that place, and that from some cause or another a cavity had previously existed in the sandy substratum, into which the upper stratum was driven by the electric force. No marks of the effects of a very high temperature were observable amongst the inflammable materials surrounding the hole. On reconsidering the circumstances, however, there appears great probability of a mass of heavy matter having fallen at this place, as lightning alone is not known to produce any similar effects.

W. S.

*Caution to Experimenters with the Electrical Kite.**

On Friday last, about half-past two in the afternoon, clouds began to form in various quarters of the heavens in rapid succession, from mere specks or streaks to immense groups, with every appearance of being highly electrical.

I repaired to the Artillery Barrack Grounds with an electrical kite, and in a very short time got it afloat, letting out string through the hands from a coil or clue, which was thrown on the ground. When about a hundred yards of the string had

* Philosophical Magazine, for October, 1834.

been let out, a tremendous discharge took place, which gave me such a blow in the chest and legs that I became completely stunned, let go the string, and consequently the kite soon fell.

The accident was owing entirely to my own neglect, and could not possibly have happined had I taken the following precaution, which I here give for the guidance of other Experimenters.

Let all the string intended to be employed be first taken off the reel, or coil, and stretched on the ground. Let now the insulating cord, ribbon, glass, or whatever is used for this purpose, be attached to the kite-string and fastened to a peg, stake, tree, or anything intended to hold the string during the time the kite is afloat. Next fasten the kite to the other end of the string, and let it ascend from the hand.

This is the manner in which I usually proceed when heavy clouds are hovering about, and ought always to be attended to, although I neglected it on this occasion. By these means the Experimenter is completely out of danger, and he may easily ascertain if the string be highly charged by going to the other end, where it is insulated, because of the brushes of light and the crackling noise that attend them.

I find it convenient to have a sliding wire on the silken cord, which can be moved to and fro, by means of a long and well varnished glass rod, to any required distance from the end of the wired kite-string—the other end being stuck fast into the ground. If the electric fluid strikes two inches over the surface of the dry silken cord (and it will sometimes strike a yard), it would not be safe to approach it; and no man could hold the string when it strikes one inch along the insulator, or through the air.

After the electrical state of the string has been ascertained to be approachable with impunity, the wire may be slided away from it as far as possible: the insulating cord ought never be less than two yards long. The other end of the wire is then to be taken out of the ground and attached to the apparatus for experiment. This done, the wire is again slided up the silken cord close to the wired-string, and there to remain during the time the experiments are carrying on.

The only method of getting the kite down, during an intense electrization of the string, with safety to the Experimenter, is to unfasten the silken cord from its hold, and let all go—the kite falls. I have frequently been annoyed whilst holding the kite-string in hot hazy weather, when no distinct cloud was visible, by a rapid succession of discharges, from which I had no other means of escape than by quitting the string, and letting the kite fall. The same thing sometimes happens whilst experimenting in cold dense fogs in the winter season. I have experienced these rattling volleys of electrical discharge when the kite has not been more than thirty yards from the ground, and the wired-string touching it at the same time. Consequently great quantities of the fluid must have passed into the ground, directly through the wire of the string, in addition to that which produced the shocks.

The publicity of these particulars may possibly prevent some inexperienced Electrician from receiving a death-blow from his kite-string.

Young persons who are fond of kite-flying should also be cautious, and not have their kites afloat during thunder storms; because it is possible that a wet string may transmit a violent discharge of lightning which would be productive of serious accidents.

W. S.

Artillery Place, Woolwich, July 23rd, 1834.

P.S.—Many other useful observations might be adduced, but as I have a volume in the press, expressly devoted to atmospheric Electricity, both theoretical and practical, the reader must excuse any further information on this subject in the present work, which has already swelled to a greater extent than was at first calculated on.

SECTION VI.

MISCELLANEOUS SCIENTIFIC INVESTIGATIONS. BY W. STURGEON.

An Investigation on the cause of the Fracture of Jars during an Electric Discharge; and on the mode of protecting them.

Perhaps no circumstance whatever tends more to damp the spirit of philosophical inquiry, or retard the progress of scientific pursuits, than that of expensive apparatus, and the liability of spoiling various parts of it by the process of experiment, without ever attaining anything like a satisfactory result. Experiments are frequently attempted, both in Chemistry and Electricity, which, if unattended by misfortune, might lead to the most important results; but the breaking of a retort or a jar in one moment defeats the object, and the Experimenter, by this unfortunate circumstance, abandons perhaps altogether the subject he was ardently pursuing. Any attempt then, however humble, that can possibly aleviate the embarrassment of the Experimenter under such painful and discouraging circumstances, must necessarily be considered of some importance, not only in any particular inquiry, but to the encouragement of scientific pursuits in general.

Electric jars, when charged intensely, it is well known are frequently preforated, or starred, as some persons call it, on being discharged. Many Experimenters break their jars by the unscientific practice of placing one of the balls of the discharging-rod against the side on the coating, whilst the other ball is made to approach that of the jar, and which communicates with the lining. For, with this disposition of the discharging-rod, it is evident that when the discharge takes place the whole force of the concentrated fluid, which before was distributed over the whole area of the lining, is now suddenly impressed on that particular point of the coating immediately in contact with the lower ball of the discharging-rod, and the glass not being sufficiently strong to sustain the shock is frequently perforated, and then (as to electrical purposes) rendered entirely useless.

Misfortunes of this nature might easily be averted by placing the jar on any good conducting substance, such as a piece of tin-foil, or on a plate of any other metal; for by placing the lower ball of the discharging-rod on the metallic plate, the fluid will be dispersed over every part of the bottom of the jar at nearly the same moment, and thence equably disseminated over the rest of the coating, without the possibility of breaking the jar by the whole force acting against any particular point. This precaution, however, although necessary to be observed whenever a jar or battery is discharged, is by no means a complete protection; for jars are frequently broken although every possible attention be paid with respect to the application of the discharging-rod: and it is well known by every person familiar with experimental Electricity, that when a large battery is discharged, one jar, at least, is almost sure to be broken; and so little has the *cause* of this phenomena been understood, that no method is yet before the public, with which I am acquainted, to remedy the expensive and highly discouraging evil. It is, therefore, a common practice with Electricians not to charge their batteries to a high intensity, lest some of the jars should be broken by the discharge.

The only direction given by authors on this subject for preserving intensely charged jars, is that of transmitting the discharge through a long circuit. But even by this precaution the end is not answered; for by transmitting the discharge through a long circuit the effect is considerably reduced, and therefore nothing more is obtained by this means than by transmitting a charge of lower intensity through a short circuit.

The frequent misfortune of breaking jars by the discharge, and the consequent expense and trouble attending the fitting up of new ones, led me to the determination of investigating the cause, and if possible to remedy the evil; and I am happy to say that this investigation has enabled me to accomplish the object to the utmost of my wishes.

Before entering on a description of the method I have taken to prevent jars from breaking by the electric discharge, it may perhaps be necessary to premise by pointing out such particulars as were noticed during the inquiry, and which seemed most worthy of attention.

The first jars used in these experiments were fitted up in the usual way, by coating and lining them to within about three inches of the top. The wire of each jar, surmounted by a ball, had its lower extremity passed through the cover or lid, and looped to a chain, the lower end of which rested on the bottom of the lining.

When jars fitted up in this manner broke by the discharge, the fracture or star always occurred at nearly the top of the lining. Now as the joining of the chain with the lower extremity of the wire was nearly opposite to the point where the fluid perforated the jar, I concluded that the chain, in consequence of the inter-

ruptions at the links, was incapable of conducting the fluid from the bottom of the jar; and that instead of the whole quantity being transmitted from the bottom to the top of the chain, the greatest portion of the collected fluid escaped by one sudden effort from the upper edge of the lining to the lower extremity of the wire; and thus nearly the whole force of the fluid being discharged by explosion at one particular point of the lining, the reaction against the side of the jar caused the latter to be broken for the same reason that jars frequently share the same fate by having the lower ball of the discharging-rod placed against their sides.

Jars were afterwards fitted up without any chain, having one continued wire from the ball on the top to the bottom of the jar, in hopes that, if the wire was sufficiently stout, the whole of the collected fluid would be transmitted in safety throughout its whole length. Hence, the explosive effort from the lining to the wire being thus supposed to be annihilated, the jar would be preserved. It was not long, however, before I discovered my mistake; for a jar, so fitted up, had not been charged and discharged more than five or six times before it was perforated by the fluid close to the upper edge of the foil. Some others were fitted up in the same manner, only using stouter wire from the ball to the bottom of the lining; still impressed with the idea that, if the wire was sufficiently capacious, the fluid would be safely conducted to the discharging-rod without injury to the jar. Some of these jars stood the discharge from high intensities extremely well, and perhaps much longer than if the wire had not been so stout; nevertheless, as some were perforated in exactly the same manner as those before described, it was evident that this mode of fitting them up was no real protection, and that some other must be resorted to before intense charges could be transmitted through short circuits with safety to the jars.

The jars used in these experiments were of green glass, and broke most frequently when several of them were used at the same time in the form of a battery. I generally used six or eight at a time. I have, however, had the misfortune to fracture several of white flint glass, one of which exposed a coated surface of nearly three square feet. This jar stood very well at moderate charges, but was perforated by the first discharge of intense electrization. It would be needless to enumerate more of these disastrous circumstances, as they occur frequently, and in the hands of every Electrician.

Now as so many jars had been broken exactly opposite the upper edge of the lining, it would seem as if the collected fluid made the greatest effort to escape from that particular part; and, if it be admitted that the particles of the electric fluid are repulsive of each other, it is only reasonable to suppose that such would be the case: for although an equable dissemination may actually take place over every other part of the lining, yet the naked part of the jar above the lining not being charged, the fluid about the edge of the foil, finding a less resistance upwards than in any other direc-

tion, would accumulate to the greatest extent around the upper edge of the lining, and consequently would have a greater tendency to strike the wire from some point near the top of the lining than from any other part of the jar.

Although there are many jars that will withstand the utmost force of a discharge, nevertheless there can be no doubt but in every jar that is fitted up in the usual way the fluid strikes the wire from the upper part of the lining in almost every discharge from high intensity; and that those jars which have not broken owe their safety to the strength of the glass. This conclusion may, I think, be fairly admitted when it is considered that many jars will withstand the shock for hundreds of times, although one of the balls of the discharging-rod be placed against the coating at every discharge, whilst many others, which are of more feeble glass, have broken at the first discharge by this unskilful application of the discharging-rod.

If due attention be paid to the nature of the electric jar, and the manner in which it is usually constructed, it will appear evident that during a discharge, if the whole quantity of fluid from the lining were to flow through the conducting channel purposely arranged for it, that portion which occupied the upper part of the lining would have to travel by a very circuitous, and perhaps injudiciously chosen route. It would first have to descend to the bottom of the jar before it arrived at the axial wire, thence the whole length of that wire before it arrived at the ball on its top. Moreover, whilst ascending the axial wire, the fluid would have to pass the very point from whence it first set out. Now the electric fluid has never been understood to evince any tendency to move by a circuitous route, and more particularly so when one portion of it has to *meet* or *pass close to* another portion in a *recurved* circuit similar to that presented by the lining and axial wire of a jar. Moreover, it appears to me that the greater part of the axial wire, which is *within* the jar, is in a *negative* condition, relatively to the lining and upper part of the stem and ball, from the moment the jar ceases to receive fluid till it is discharged; and this negation would be increased for a moment by the presentation of the discharging-rod to the ball of the jar, and thus place that part of the axial wire in very ample condition for the reception of the fluid from the most vicinal and intensely-charged point of the lining, which, as already assumed, would be in some part of its upper edge. There can be no just reason to doubt, however, that some part of the charge actually makes its escape by the chain from the bottom of the jar; but the proportion of fluid discharged by this route is probably very small, and especially in very tall jars, such as are most frequently selected for electrical purposes, where the fluid in some cases would have to traverse two or three feet of lining, chain, and wire, and yet be no nearer the ball on the top of the jar than it was before the approach of the discharging-rod.

Now since the conclusions drawn from this investigation show that the electric fluid in the interior of an intensely charged jar indicates the greatest tendency to

escape from the top of the lining, and that by the present mode of fitting up jars an explosion from that part of the lining to the wire probably takes place whenever the jar is discharged through good conducting media, it seemed the most natural method to *lead* the fluid, as it were, by some good conducting substance by the nearest route, and dispense altogether with the wire and chain that are suspended in the axis of the jar. For this purpose two slips of tin-foil were secured to the opposite sides of the jar, and reached from the upper edge of the lining to the cover on the top; the under part of which was also covered with foil. This latter portion of foil communicated with the lower extremity of the wire which supports the ball, so that a complete and direct metallic connection now existed between the top of the lining and the ball on the top of the jar; therefore no explosion in the interior of the jar could possibly take place. The result was that every jar so fitted up has hitherto withstood the most severe trial. I have, for the last twelve years, employed jars thus protected without ever breaking one by a discharge, although, during that period, I have discharged a battery of twelve jars some hundreds of times from the most intense electrization. I have called the slips of foil *protectors;* and I am firmly persuaded that the most extensive battery may, by this means, be perfectly protected.

The next question that naturally presents itself is, does the insulation continue as perfect in jars thus protected as in those fitted up in the usual way?

Upon attentive examination it will be found that the ball and stem of the ordinary jar must always be electrized almost equally with the lining with which they are connected by means of the chain, although the chain itself may be rendered negative by by the surrounding forces of the lining; and therefore the insulation from the lining to the coating in such jars can only be from the centre of the cover, through which the wire passes, to the upper edge of the coating; and as the cover is of wood, which is always a partial conductor, it also becomes charged in common with the wire and lining. Therefore, the only perfect insulation is that between the edge of the cover and the top of the coating, exactly the same as in jars furnished with protectors.

When jars are cylindrical throughout, no covers need be used. A disc of wood, of nearly the same diameter as the interior of the jar, is fixed by wedges of cork at the same height as the top of the lining. This disc is covered with tin-foil, and the lower end of the wire carrying the ball is screwed into its centre. In addition to the cork wedges, I usually support the disc by three wooden rods, which rest on the bottom of the jar.

It is not my intention, however, to press the theoretical part of this paper too strongly on the attention of the Society; because I am well aware that unless experiments were made to show that the fluid actually leaps from the upper edge of the lining to the axial wire of the ordinarily fitted up jars, it might lead to unnecessary doubts in the minds of those who have paid but little attention to the pursuit of a cause whose effects are the most disheartening that the amateur Electrician has to contend with.

I have pointed out what has appeared to me to be the cause of these accidents to jars, and have briefly described the mode of investigation, both mental and experimental, which I pursued; and whether my theoretical views be considered satisfactory or otherwise, the simple fact alone of my not having broken even one jar thus fitted up, although I have constantly employed them for the last twelve or more years, may perhaps be sufficiently important to induce other Electricians to adopt the same mode of protecting *their* jars, which for so long a course of practice has afforded a complete protection to mine. If I have succeeded in this particular, the principal object for offering this paper to the notice of the Electrical Society will then be accomplished. W. S.

Westmoreland Cottage, January 3rd, 1838.

Two Brilliant Electrical Experiments, well calculated for the Lecture Table.

The first of these experiments to be described is made by the electrical machine and the apparatus represented in Figs. 6 and 7, Plate XVIII.; the former being a front and the latter a side elevation.

A A is a stout rectangular mahogany board, which is the base of the instrument. B B B is a vertical piece of similar board, the lower end of which is firmly fixed in the base. Near to the upper end of this vertical piece is a crutch, C, Fig. 7, which, together with the main upright, B B, carry a spindle with its pully, P. The spindle and pully are put into rotatory motion by means of the wheel, W W, and its band. The farther end of the spindle terminates in a hollow brass ball, into the opposite sides of which, and at right angles to the axis of the spindle, are cemented two glass tubes, spirally spotted with tin-foil, as seen in Fig. 6. The outer end of each tube terminates with a small brass ball. By this arrangement the spotted tubes can be put into rapid rotation in a vertical plane. S is a glass pillar, surmounted by a brass socket, terminating upwards in a screw. On this pillar is screwed the ring, O O O, made of stout brass wire. The inner diameter of the ring must be a little greater than the distance between the outer surfaces of the balls terminating the spotted tubes, in order that the latter may rotate within the ring without touching.

The axis of motion is in the centre of the ring, and perpendicular to its plane. A horizontal wire, *w*, terminating with a brass ball, is screwed to, and projects from, one side of the ring.

When an experiment is to be made with this apparatus, the ball, *w*, is to be brought close to the farthest extremity of the prime conductor of an electrical machine in good order, or to a ball preceeding from the conductor; and in this position the base-board is to be screwed firmly to the table, in the usual way, with clamps.

When the machine is at work, sparks will pass from the prime conductor to the ball, *w*; and again from the inner side of the ring to one or both of the spotted tubes, which will be brilliantly illuminated, especially if the spindle be touched with the hand, or connected with the cushion by a copper wire. Let now the wheel, w w, be gently turned: the spotted tubes will still be illuminated, but instead of showing stationary spiral lines of fire, they will now exhibit the most pleasing spectacle ever beheld in the whole range of electrical illuminations, whose fantastic forms will undergo a variety of changes with the speed of the wheel; and, when the velocity is considerable, the optical illusion creates ideas of a complete disc of electrical light.

This splendid experiment is susceptible of much pleasing variation. If, instead of having the tubes cemented into the central revolving ball, they be fastened to it with screws in the usual way of screwing balls on the extremities of wires, they may easily be removed and replaced by other devices, such as tubes of coloured glass spotted in the same manner, or by slabs of plate glass, spangled in the usual way and varnished with different colours. In this way a disc of any coloured light may be composed of concentric annular portions, each of a different colour. If, for instance, the faces of two revolving slabs of glass were each divided into three equal portions of lines perpendicular to their edges, and that the inner portion be varnished yellow, the middle red, and the outer portion blue, each portion would form an annulus of its own colour, and the whole would fill up the whole disc. In all experiments with this apparatus, however, the disc, whatever colour or colours it may exhibit, will necessarily appear annular, because of the central brass ball, which, in a darkened room, is a complete black speck.

By this apparatus differently coloured pieces of glass may be made to combine the light they transmit, and the composition of colours displayed in the most splendid manner.

The second experiment is performed by the aid of a magnetic electrical machine and the apparatus represented by Fig. 17, Plate XI. which is an end view.

In all magnetic-electrical machines made in London, the coil wires terminate in cylinders concentric with the axis of motion, or spindle, which carries the revolving cross-piece with its coils; which cylinders are imperfectly insulated from each other by an intervening cylinder either of box-wood, ebony, or some material of an imperfect conducting character.

In Fig. 17 the central white space represents the end of the inner cylinder, usually a piece of stout copper or brass wire, to which the inner ends of the coil wires are soldered. The shaded annulus next this cylinder is the hollow insulating cylinder of wood, and around it is seen the end of another unshaded annulus representing the outer brass cylinder, to which the outer end of the coil wires are soldered. These cylinders fit one another pretty tight, to prevent their moving out of their places.

Instead of making the usual connections for showing the spark, a piece of iron wire is screwed into each metallic cylinder, pointed at the other end, and bent nearly at right angles, as seen in the figure; and, when the machine is in motion, these wires are carried round in a vertical plane in front of, and parallel to, an annular iron disc, *i i*, which is firmly fixed in the base-board of the machine by the pillar, s. The points of the iron wires are directed obliquely towards the iron disc, against which they press gently during the whole revolution. By this means the two cylinders, and consequently the ends of the coil wires, find a metallic connection which is frequently interrupted whilst the points are scraping over the asperities on the surface of the iron disc, and two concentric circles of brilliant scintillations are thus produced. This is, perhaps, the most pleasing experiment yet shown in Magnetic Electricity.

By this arrangement it sometimes happens that the revolving points do not succeed one another properly, in the contacts with the iron disc, in order to produce complete circles of scintillating fire. When this is found to be the case, it may be necessary to remove the longer wire from the outer cylinder, and connect the latter with the iron disc by a piece of copper wire, having a loop at one end for the reception of the cylinder, and bent into a hook at the other to hang on the inner edge of the annular iron plate. This wire, however, is a mere substitute for a spring of either steel or brass wire, one end of which is screwed tight to the back part of the iron disc, and the other presses against the outer revolving cylinder. This spring can be turned off the cylinder at pleasure by slackening the screw in the iron disc, and replaced by the iron point, as in the figure.

The iron disc, *i i*, may be either rough or smooth, bright or dull; and as much of the display depends upon the combustion of the ferruginous matter, steel points are preferable to iron ones. If the points be filed or ground very fine, each wire may carry two, at different distances from the centre of motion, which will usually produce four concentric circles of fire.

If, instead of a steel point, one of platinum be used, leaves of laminated metals, covering the face of the annular disc, *i i*, will be deflagated. Gold, silver, and the Dutch metals will, by this means, respectively display the same characteristic coloured light as by the action of a Voltaic battery. The metallic leaves will be sufficiently attached to the plate, *i i*, by first breathing on the latter, and then pressing the former against it. In these experiments, however, it is necessary that the plate, *i i*, be bright: it may be of iron or any other metal.

The deflagration of mercury is beautifully shown by an apparatus of this kind. Let the annular disc, *i i*, be of copper, and the whole face over which the points revolve slightly grooved with a file in close radiations from the inner to the outer edge. Let this radiated face be amalgamated with a dilute solution of nitrate of mercury, then washed in clean water, and afterwards covered with clean mercury, so that the whole

face may appear bright. Now remove the surplus mercury from the grooves with a soft tooth brush, or with a stout feather, and the disc is ready to be attached to the machine for an experiment. If the revolving points be amalgamated copper, two circles of brilliant white light will be exhibited; but if they be iron or steel, the colour of the luminous circles will be a compound of the white light from the mercury, and of the fiery red from the deflagrating ferruginous points. If points of zinc, gold, silver, &c. be used, each metal will tend to modify the tint of the luminous rings. Thin wires of these metals, tied by binding wire to the revolving wires, answer extremely well for the points.

W. S.

Curious Voltaic Experiment.

We are indebted to Mr. Porrett for the discovery of a disturbance of the common hydrostatical level of water by Voltaic action. Mr. Porrett's interesting experiment is well described in the 8th volume of the *Annals of Philosophy*. The arrangement consisted of a glass vessel divided into two compartments by a bladder partition. "One of these compartments having been filled with water, and left for several hours, was found to have retained the water. The bladder, therefore, was not sufficiently porous to allow the water to filtrate through it. The cell filled with water was now positively electrified with a Voltaic battery of eighty pairs of $1\frac{1}{4}$ inch double plates, and a few drops of water were put into the empty cell, so as to cover the bottom of it. This small quantity was then negatively electrified" by placing in it the wire from the negative pole of the battery. "Independently of the decomposition of a small part of the water, which of course took place in the usual manner, the principal part of it obeyed the impulse of the Voltaic current, *from the positive to the negative wire*—first overcoming the resistance occasioned by the compact texture of the bladder, so as in half an hour to have brought the water in both cells to a level, and afterwards overcoming the additional resistance occasioned by the gravitation of water, by continuing to convey that fluid into the negative cell until its surface was upwards of three-fourths of an inch higher than in the positive cell."

This experiment was repeated by M. de la Rive and Mr. Mullins with similar results;* but the former gentleman has stated that he could not succeed unless distilled or river water was placed in the cells, for when he employed a saline solution of a proper strength no such effect of impulsion was perceptible.†

Having ascertained the correctness of the statements of Mr. Porrett, I became desirous of knowing whether or not a feebler Voltaic intensity would produce a similar effect by a longer continued action; and as earthenware diaphragms are now much

* Annals of Electricity, &c. vol. i. page 206. † Ibid.

employed for separating the copper from the zinc of a Voltaic pair, I employed an unglazed stone-ware jar, which was kindly given to me by Mr. Gassiot, for a first trial. This jar is about six inches high and three and half diameter. It was partly filled with a solution of carbonate of soda, and placed in a larger jar of porcelain, which was afterwards filled about half way up with a solution of sulphate of copper. A cylinder of rolled copper, sufficiently large to envelop the diaphragm, was placed in the sulphate of copper, and a small cylinder of zinc, in metallic connection with the former, was placed in the solution of soda. The liquids inside and outside of the stone-ware diaphragm were of precisely the same altitude. In three days a difference of altitude in the two liquid columns was very perceptible; and in about ten days from the commencement of the experiment, nearly the whole of the liquid in the zinc cell was driven through the stone-ware partition into the outer compartment. The copper solution was so completely decomposed that scarcely a trace could be detected by liquid ammonia; and but a very slight tinge was given to litmus paper, showing that the acid liberated from the sulphate had been either neutralized by the soda or decomposed by acting on the zinc; or its disappearance was probably occasioned on both these accounts. A thin crust of salt (probably sulphate of soda) covered the moisture which was left in the bottom of the diaphragm, and the zinc was much corroded and partially covered with a white matter, partly soluble and partly insoluble in water, the latter portion being a carbonate of the oxide of zinc. I have repeated the experiment with precisely the same results, obtaining a difference of altitude of four inches in the two liquids, the filtration being always in the direction of the electric current. I have also obtained similar electro-transportations of liquid media through diaphragms of common tile and soft red brick, and I have no doubt of their being obtainable by sandstone and even thick masses of granite.

W. S.

Description of a new Galvanic Battery.

The battery I am about to describe will be found exceedingly useful either for display on the lecture table or an as an implement of research. It consists of a rectangular wooden box, as represented at the bottom of Plate XVIII. in which are cemented a series of Voltaic pairs of copper and zinc, in the Cruickshank form. Every cell formed by these metals is divided into two compartments by a diaphragm of mill-board, also cemented into the sides and bottom of the box, so as to be completely water-tight. The figure represents six metallic pairs and five diaphragms, the former by the *light* partitions and the latter by the *shaded* ones. As the mill-board is pervious to aqueous liquids, it is obvious that any two kinds may be employed which

may be found most suitable to operate with, the one on the copper side and the other on the zinc side. Twelve plates, six inches square, answer very well for decomposing acidulated water, when a solution of sulphate of copper occupies the copper cells, and a dilute solution of soda the zinc cells. Such a battery will be found to possess many recommendations. W. S.

Account of some Magnetic Electrical Experiments made by means of an Original Discharging Apparatus attached to the Large Magnet, at the Exhibition Room, Adelaide Gallery, Lowther Arcade, London.

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,—Having obtained permission of the proprietors of the Exhibition Room, Adelaide Gallery, to employ their large magnet in any new experiments that I might wish to undertake, I availed myself of that privilege for the first time on Thursday evening, the 28th of August last. The following results were produced:—

The decomposition of hydriodate of potassa in solution—first, by paper moistened in it, and placed on a platinum plate attached to the negative side of the circuit, and occasionally closing the circuit on the upper side of the paper with a platinum wire which proceeded from the positive side, the machine being at work during the time: at each touch of the wire with the paper, however short the time of contact, iodine was evolved at the positive point of the platinum wire.

Second, a solution of the hydriodate and starch was placed in a rectangular glass box, with a gauze partition to divide it into two compartments. A platinum plate, properly connected with the machine, was placed in each compartment. In half a minute the positive cell was completely obscured by liberated iodine. A more striking experiment was never exhibited. Platinum wires, properly arranged in the circuit, were placed in solution of acetate of lead: the metal was revived on the negative wire.

Solution of sulphate of copper was also subjected to the action of the current: decomposition immediately took place, and the negative wire became completely covered with copper.

Water was also decomposed, the hydrogen and oxygen being collected in separate tubes.

The above experiments were repeated by reversing the direction of the current, and the results were exhibited as decidedly as before, and with the same promptitude as by the employment of a Voltaic battery.

Hard steel was also magnetized by being placed in a helical part of the conducting wire, and the poles were reversed at pleasure by changing the connections.

A soft iron horse-shoe was magnetized to nearly as high a degree as if placed in the circuit of a Voltaic battery.

I have also made a variety of electro-magnetic rotations, and some other rather novel motions by magnetic-electrical currents, which I intend to publish as soon as opportunity permits.

Your very obedient Servant,

Artillery Place, *Woolwich*, WILLIAM STURGEON.

Oct. 15*th*, 1834.

P.S.—I beg permission thus publicly to acknowledge the obligations under which I am placed for the very handsome manner with which Mr. Payne, Superintendent of the Gallery, undertook to procure me the use of the magnet; and for the very able assistance of Mr. Maugham, Chemical Lecturer, whilst carrying on these novel experiments in this department of Electricity. W. S.

Description of three different Instruments for Opening and Shutting the Battery Circuit of an Electro-Magnetic Coil Machine.

First Instrument.—In the usual mode of opening and shutting the battery circuit by the wires of a revolving iron magnet, between the poles of a permanent horse-shoe magnet, there never can be more than two interruptions during each revolution of the iron. These interruptions of the circuit occur at the partitions of the vessel, which divide it into two cells for holding the mercury as poles to the battery. It is thus that the sparks are exhibited at two points of the circuit only, both of which are in the plane of the vertical horse-shoe magnet; and, for the same reason, only two shocks can be given from a coil machine for each revolution of this kind of discharger. Hence it is that this instrument is not well calculated either for the exhibition of sparks, or for the discharge of a rapid series of shocks; for the latter purpose, however, the object might be accomplished by employing an active battery, which would produce a more rapid rotation of the electro-magnetized iron. Notwithstanding these defects in the instrument, however, it is, without exception, the most convenient mode of opening and shutting the battery circuit, either for medical or other purposes, where an assistant is not at hand, or whose company is not wanted, that has yet appeared before the public.

The instrument I am about to describe I hope will be found to possess some advantages over that just mentioned, and can hardly fail to become generally employed,

both at the lecture table and as a useful appendage to the electro-magnetic coil machine when used for medical purposes. Figs. 2 and 3, Plate XVIII. respectively represent a horizontal and vertical plan of the instrument. The part, *b b b*, represents a round wooden base, in the upper part of which is an annular groove for the reception of mercury. The surface of the wood in the centre of the groove is in the same plane as the outside rim, and about one inch diameter. The groove is about three-quarters of an inch broad and half-an-inch deep. It is separated into two compartments by two low wooden partitions, *p p*, which are glued to the bottom and sides of the groove, diametrically opposite to each other, and are about two-tenths of an inch high. Besides these partitions there are two *half*-partitions, one in each compartment. These half-partitions are glued to the bottom and inner edge of the groove, and project a little more than half-way across it. Across the under side of this box, and parallel to the two whole partitions, is cut a groove for the reception of the central part of a staple-shaped steel magnet, *n g s*, whose extremities rise vertically on the opposite sides of the instrument to about an inch and a half above the surface of the wood.

A piece of cylindrical rod iron, about four-tenths of an inch diameter, and a little shorter than the distance between the magnetic poles, having a pivot in its centre, is covered with a coil of varnished copper wire, whose extremities hang down into the groove when the iron is properly placed on its pivot in the centre of the instrument. The iron, thus mounted on its pivot, is susceptible of rotatory motion in the plane of its own axis, between the poles of the permanent steel magnet, *n s ;* and whilst revolving, the extremities of the coil wire just skim the partitions and half-partitions without touching any of them.

When this instrument is used, mercury is to be placed in its two compartments until its surface will admit the immersion of the depending points of the wire which covers the iron, without its covering the partitions and half-partitions over which the points have to travel. The ends of the wire which dip in the mercury are to be amalgamated previously to the mounting of the iron. The poles of the battery are to be connected by wire with the two portions of mercury in these indented compartments, one with each, observing that the coil to be employed be properly placed in the circuit.

With this instrument the mounted piece of iron will rotate upon the same principles as that in the one hitherto in use ; but instead of having only two places in the circuit for the exhibition of sparks, we have here four places, because of the circuit being opened at both the *whole* partitions and at the *half*-partitions ; moreover, when the *half*-partitions are properly disposed in the compartments, each of them will cause *two* interruptions of the battery circuit in one revolution of the iron, and consequently *four* interruptions are obtained from *both* half-partitions. Hence the whole number of interruptions in one revolution is six ; and as a spark or a shock is obtained at each

interruption, we have six sparks or six shocks, or both, produced for each revolution of the iron. By employing two more half-partitions properly disposed in the compartments, one in each, we obtain four more sparks or shocks in one revolution of the iron. By employing two more half-partitions in each compartment we obtain ten sparks and ten shocks in the same time that two would be obtained by the old instrument. Moreover, there being twice the number of sparks at the half-partitions of those exhibited at the whole partitions, the light is proportionably brighter; and as they are not in the least concealed by the poles of the magnet, they are seen to much greater advantage than those in the plane of the magnet. With *one* jar battery and a good coil, the sparks exhibit a very beautiful spectacle. With four or six jars in series, the group of electric stars are really grand.

I have never yet used more than six half-partitions, but it is obvious that they may be multiplied much farther; each pair giving four sparks or shocks for every whole revolution of the iron. In point of exhibition, the apparatus is much improved by having a glass vessel for the mercurial compartments; and by having three or four half-partitions in each compartment, the dish is filled with brilliant electric light, emanating from a circle of refulgent stars, which dance on the mercurial margins in the most fascinating and imposing manner; whilst the singular noise which attends their exhibition indicates the rapid transmission of the subtle element through the intervening resisting medium. Another form which I have given the apparatus shows the sparks at only *one* of the points of the revolving wire.

Second Instrument.—In the first volume of the *Annals of Electricity* (see fifteenth Memoir, page 306), I described an apparatus consisting of a notched zinc disc and a revolving spring which passed over the notched surface, exhibiting a circle of sparks whose number in each revolution of the spring was equal to the number of notches in the disc. The zinc disc was soldered to a notched copper disc a little larger than itself, and the copper disc to a notched one of iron still larger than the copper, forming altogether a kind of metallic rosette, so that by having springs of different lengths, corresponding to the different distances of the metals from the centre, the colour of the light exhibited on the respective metals was easily shown. Since that time I have modified this apparatus in two different ways, which I will now describe :—

The first modification of this instrument consists chiefly in substituting rings of the various metals for the discs in the original instrument. These rings consist of iron, copper, brass, zinc, tin, lead, bismuth, and antimony, which fit one into another, and are soldered together in the order they are named, constituting one compound annular disc, which is notched in eight radii from the outer to the inner metal, as represented by Fig. 4, Plate XVIII. When this instrument is employed, the different spring pieces are of the same kind of metal as those against which they rub on the face of the disc. They are fixed at one end, in a flat hole in the revolving axle, by means of a

screw with a milled head, the outer ends pressing against the face of the disc. With this apparatus the spring pieces have to be changed with every variation of the experiment in precisely the same manner as with the original instrument; but the rings forming the compound disc admit of a greater variety of metals than by having a complete disc of each kind, and the instrument is much more elegant. Finding also that the sparks are much finer when the spring moves over the disc at a moderate than a great speed, I have done away with the multiplying wheel and bands, which are always troublesome to keep in order; so that now the revolving springs are attached to the same axle as the handle or winch is attached to.

Third Instrument.—This form of the apparatus affords a better opportunity of comparing the different coloured light exhibited by the deflagration of the various metals on the face of the disc than any other form I have yet given to it, because of the deflagration of the whole of them going on at one and the same time. The face of the disc over which the springs pass is represented by Fig. 5, Plate XVIII. The disc itself is of brass—its face being studded with those metals whose electro-deflagrations are intended to be exhibited. The metallic studs are arranged on the face of the disc in two semi-spirals, as seen in the figure. These studs are soldered to the face of the disc, and rise about a quarter of an inch above its plane, or stand in relief about that much. These studs are of the following metals, and stand upon both sides of the face of the disc, from the margin towards the centre, in the order they are named. They are iron, *i*; copper, *c*; brass, *b*; zinc, *z*; silver, *s*; antimony, *a*; tin, *t*; bismuth, *b*. The initial letters indicate the positions of the respective metal. The disc is fixed vertically to a pillar attached to the coil apparatus; and a spindle, *a*, furnished with a winch behind, passes through the central opening to some distance in front. To that part of the spindle projecting in front of the disc, and at right angles to it, is attached a metal arm, which carries eight steel springs. These springs are fixed to the arm at distances from the centre of motion corresponding to those of the metallic studs, which they are intended to touch. The outermost spring is steel throughout, and reaches to the iron or steel studs, *i i*. The next spring is tipped with copper, and reaches to the copper studs, *c c*. The next spring is tipped with brass, and reaches to the brass studs, *b b*. The next spring is tipped with silver, and reaches to the silver studs, *s s*; and so on for all the rest. The springs are fixed to the arms in one plane, which passes through the plane of its axis.

With this arrangement it is obvious that if the springs be rotated in the direction of the arrows, and commence from a vertical plane, one of the iron or steel studs will be the first at which the circuit will be opened. Hence a brilliant fiery scintillation will be seen at the edge of that stud; but the moment that the steel spring has quitted its fellow stud, the copper spring comes in contact with the copper stud; and when the copper spring has quitted its stud, the brass spring comes in contact with

the brass stud: and in this manner the springs open and shut the circuit with their respective studs in regular succession, from the outermost to the innermost metal, through one-half of the circuit. When one-half of the revolution is completed, the other series of studs are brought into play, closing and opening the battery circuit in the same regular succession as the former series.

When the springs revolve over the studs in the opposite direction, the series of metallic contacts are in the reverse order.

No language can convey a proper idea of the beauty of an experiment with this apparatus when attached to a good coil battery. It must be seen to be understood.

W. S.

*Experiments on the Decomposing Power of an Electric Current, when Platinum Terminals of different dimensions were placed in the same Circuit; also Experiments for Ascertaining how far Electro-Decompositions and Deflections of a Magnetic Needle, by one and the same Current, correspond with each other.**

Two decomposing instruments were placed in sequence in the circuit of a magnetic electrical machine:† the terminal metals in the two instruments were platina-foil, but of very different dimensions. Those in one apparatus were each one inch high and a quarter of an inch broad, and those in the other were four inches high and two inches broad; consequently the platina surfaces exposed to the acid solution (sulphuric acid and water) in the two decomposing instruments were as 1 to 32. Both the oxygen and hydrogen gases were collected from the two pairs of terminals in the usual way; and the volume from the small terminals was to that from the large ones as 10 to 7. Several trials were made, and the difference was never found to be less, but seemed to vary with different velocities of the revolving coils. No error could arise from calculation, because the collecting tubes were of the same dimensions, and the altitudes of the columns of gas had only to be measured to ascertain their relative proportions. In some previous experiments, copper terminals had been employed in the larger decomposing apparatus, and the hydrogen alone collected. These copper terminals were each about three inches high, and one and a quarter broad. When placed in the same circuit with the small decomposing apparatus, the hydrogen collected in the latter was to that in which the copper terminals were placed nearly as 2 to 1—certainly never less than as 3 to 2.

* These investigations were suggested whilst perusing Dr. Faraday's seventh Series of Researches.

† By this machine about one cubic inch of the two gases is liberated from acidulated water every five minutes that it is kept in action.

Similar experiments have been made with a *couronne des tasses* of nine pairs, and the large and small platina terminals already mentioned, the hydrogen alone being collected. The volume from the small terminals was to that from the large ones as 4 to 3.

The experiment was continued for several hours for each of two successive days, and the gas in both tubes carefully collected and measured once in about every two hours during the whole time, and the proportions never found to vary in any sensible degree. At the end of the first day's work, the gas was well shaken off both the large and the small terminals, and both portions carefully measured. It was again measured in the morning, and not the slightest loss could be discover; hence no absorption had taken place during the night. The experiment was carried on about eighteen hours during the two days. A Galvanometer coil was placed in the circuit all the time. The deflection fluctuated between 15° and 30°.

Similar experiments have been made with more powerful batteries, and with results nearly the same as those last given, but varying a little according to circumstances connected with the circuit. Under some circumstances the proportions were as $3\frac{1}{2}$ to 2; and in other experiments as 9 to $5\frac{1}{2}$. When the water in the small gasometer was mixed with a little sulphuric acid, and that in the large one free from all acid and saline matter, the hydrogen in the former to that in the latter was generally as 20 to 13; and this being the case when the gas collected amounted to many cubic inches, the difference in the two volumes cannot be accounted for by any idea of absorption.

I am not aware that any experiments could be devised which would be likely to show more satisfactorily than those last described have shown, that the extent of decomposition, by any electric current, may be considerably modified by varying the extent of the terminal metallic surface in connection with the fluid compound; and that even the *same* current, in different parts of the circuit, is productive of different degrees of chemical action *inversely* as some function of the transverse sections of the fluid part of the circuit undergoing decomposition. But the deflections of the magnetic needle, we may here repeat, are in the reverse order; or, when a fluid conductor (not in the battery) is in the circuit, the deflections are *directly* as some function of the transverse sections of the fluid conductor; the distance betwen the terminal metals being constant. This is particularly the case with a single pair of metals (see fifth Memoir, page 155), and may here be remarked, that the deflections in these cases amount to an extent that would indicate the existence of an abundant transmission of fluid, yet no decomposition is perceptible.

It is obvious, moreover, from these facts, that a considerable portion of the electric currents which, between the small terminals, was employed in the decomposing process, traversed the larger liquid section as a mere conducting channel, being totally

unoccupied as a decomposing agent. From this circumstance, it would appear probable that *some* portion of every electric current, traversing a fluid conductor, is merely and inefficiently transmitted, without being in any way concerned in the decomposing process; and, as different compounds conduct with different degrees of facility, we have this obvious inference—different compounds, suffering decomposition, transmit *inefficiently* different portions of the electric matter. This inference, however, is decidedly at variance with Dr. Faraday's views of the subject, who considers that the extent of decomposition is proportional to the quantity of the electric matter transmitted.

Several series of experiments will now be placed before the reader which will be found highly corroborative of some inferences already drawn in these remarks. In every series of experiments a Galvanometer and gasometer were placed in the circuit, and in some series two of the latter instruments, but with similar terminal metals.

The results expressed in the first, second, and third tables, were obtained from a *couronne des tasses* of ten pairs of copper and zinc, excited by a solution of sulphate of copper.

Table I. shows every particular at the end of each successive five minutes, during the whole period of the first series; and is a specimen of the mode by which the *mean* of each succeeding tabular series was obtained.

TABLE I.

Magnetic Deflections and Chemical Decompositions by the same Electric Currents.

Number of pairs.	Time of action.	Hydrogen gas collected.	Mean deflection of the needle.	Series of experiments.
10	In 1st 5′	4	32°	
	2nd 5′	3	31·5°	
	3rd 5′	2	30·5°	
	4th 5′	2	28°	1st
	5th 5′	2	27°	
	6th 5′	$1\frac{1}{2}$	26·5°	
	In 30	$14\frac{1}{2}$	29·25°	

From similar series of experiments the following results were obtained:—

TABLE II.

Magnetic Deflections and Chemical Decompositions by the same Electric Currents.

Number of Pairs.	Time of action.	Hydrogen gas collected.	Mean deflection of the needle.	Series of Experiments.
7	30′	10	24°	2nd
9	30′	9·5	23·5°	3rd
	30′	7·5	21·5°	4th
	30′	7	20°	5th
5	30′	2	11°	6th
	30′	2	11°	7th
7	30′	4	20°	8th
	30′	6	20°	9th
	30′	6	20°	10th
7 pairs in action, but a wire joined the 3rd and 8th cups.	30′	2	10·5°	11th
	30′	2	10·5°	12th
	30′	1	10°	13th
4	30′	1·5	9·5°	14th
	30′	1	9·5°	15th
	30′	2	9·5°	16th
	30′	1·25	9·5°	17th
9	30′	2·75	13·5°	18th

In the above experiments it is to be understood that the needle never deviated more than 2° on either side of the mean deflection given in the table.

In the following series of experiments two electro-gasometers, in sequence and with similar terminals, were placed in the circuit, and gas collected from one only; and the needle never permitted to vary during the whole time of each series.

TABLE III.

Number of pairs.	Time of action.	Hydrogen gas collected.	Mean deflection of the needle.	Series of experiments.
9	30′	6	20°	19th
	30′	1·25	10°	20th

The next series of experiments were made with a Cruickshank's battery of fifty pairs of six-inch plates, excited by salt and water—sometimes using the whole, and sometimes only a part of them: one gasometer in the circuit.

TABLE IV.

Large battery of 50 pairs. Salt and water.

Number of pairs.	Time of action.	Hydrogen gas collected.	Mean deflection of the needle.	Series of experiments.
45	30′	12	25·5°	21st
	30′	12	22°	22nd
50	30′	6	15·9°	23rd
	30′	5	14·75°	24th
	30′	5	13·17°	25th
50	120′	16	11°	26th
	rate per 30′	4	11°	27th

The next series of experimental results were obtained from the same battery, the exciting liquid not being removed since the previous day. The action was made a little brisker by the addition of very dilute sulphuric acid.

TABLE V.

Number of pairs.	Time of action.	Hydrogen gas collected.	Mean deflection of the needle.	Series of experiments
50	30′	2.5	10·75°	28th
	30′	3	11°	29th
	30′	3·5	11°	30th
	30′	2·5	10·5°	31st
25	30′	1·5	6°	32nd
	30′	1·5	6°	33rd

TABLE VI.

With two gasometers in the circuit: gas measured in one only.

Number of pairs.	Time of action.	Hydrogen gas collected.	Mean deflection of the needle.	Series of experiments
50	30′	11	22°	34th
	30′	7·5	17°	35th

The results exhibited in the next table were obtained from a *couronne des tasses* of nine pairs. The zinc amalgamated, and excited by a very weak solution of sulphate of copper: a wire joined the fourth and seventh pair for the purpose of giving the needle a deflection of 10°, which was kept nearly steady during the whole time.

TABLE VII.

One Gasometer.

Number of pairs.	Time of action.	Hydrogen gas collected.	Mean deflection of the needle.	Series of experiments
9	30′	1	10°	36th
	30′	1	10°	37th

The experimental results which these tables exhibit require no further comment. It will be sufficient to observe, that they lead to conclusions very different to those stated in Dr. Faraday's *Seventh Series*. W. S.

Description of an Electro-Magnetic Engine for Turning Machinery.

In Fig. 1, Plate XVIII., A A A A represents a stout square board, which forms the base of the engine. In two opposite corners of the base-board are fixed the two upright pillars, B B, which carry a cross-piece, C C. In this cross-piece are fixed two other smaller pillars, P P, which also carry a cross-piece. In the centre of the engine is a vertical shaft, which turns freely in two metallic collars, one of which is in the centre of the base-board and the other in the centre of the cross-piece, C C.

About half-way up the shaft are two circular channels, one above the other, as seen in the figure. Through the centre of these channels, and at right angles to their planes, the shaft passes, and is fixed to them. Lower down, the shaft passes through the centre of an opening in the cross-piece, *d d*, also supported by two short pillars. On this cross-piece, and concentric with the shaft, are fixed four quadrantal metallic plates, separated from each other by narrow radial openings.

Near the top of the shaft, and at right angles to it, is fixed a compound bar magnet, N' S', each magnet of which is about eighteen inches long, one inch broad, and half-an-inch thick. Near to the bottom of the shaft is fixed another similar compound bar magnet, N S, with its poles in the opposite direction to the former.

In a circle concentric with the shaft, and at an equal distance from each other, are fixed in the base-board the lower extremities of four cylindrical bars of soft iron, *i i i i*, each of which is inclosed by six coils of copper wire. The coils round each cylinder are separated from each other by intervening cases of oil silk. Each set of extremities of these copper wires is soldered to one stout copper wire; hence the extremities of the twenty-four coils terminate in eight of these latter wires, four of which proceed from the lower extremities of the coils, and are soldered to the four quadrantal metallic plates, one to each. The other four stout wires proceed from the upper parts of the coils, and terminate by proper connections in the circular channels, which are partly filled with mercury. Through the sides of the channels pass four metallic stems, two through each, their inner extremities being in contact with the mercury in their respective channels. The stems of each pair are placed at 90° from each other, and the whole at right angles to the shaft. The right angle which the upper pair forms is on the opposite side of the shaft to that formed by the lower pair. From each stem hangs a metallic wire, reaching obliquely to its respective quadrantal plate on the cross-piece, *d d*, which maintains a connection between these plates and the mercury

in the circular channels—transferring the electric current from one plate to another, and consequently from one coil to another, in their progress of revolution.

To prevent the figure being distorted, none of these connecting wires are drawn. The engine is put in motion by the application of two cylindrical Voltaic batteries of a single pair each, the metals being placed in two porcelain jars, each of which holds about three pints. These batteries are connected with the conductors of the engine at their terminal cups. One battery at each end of the lowest cross-piece, *d d*.

The connections being properly made, the iron cylinders become magnetic in succession, and by the joint attractive and repulsive forces of the permanent magnets, N' S', N S, and the temporary magnets, *i i i i*, the former, with the shaft and appendages, are pulled and driven round, the action being carried on in the following manner.

Imagine that the pole, N', of the permanent magnet is placed directly between the poles, *s* and *n*, of the temporary magnets: it will, by this means, be attracted by the former, and at the same time repelled by the latter. Hence it will be urged by both these forces towards the pole, *s*. If now the contrivance be such that the Voltaic connections be broken just before the pole, N', arrives at *s*, the extremity, *s*, of the iron bar will become neutral; but the momentum of the machine will carry the pole, N', to beyond this neutral point. Now conceive that the pendent wires have been carried from their last quadrantal plates to the next in succession. The currents by this means have been reversed in all the coils, and a corresponding inversion of polarity has taken place in the vertical iron bars; hence when the pole, N', has just passed the first bar, and whilst still in motion by its acquired momentum, it will again be urged on by two other forces in a similar manner as by the two first; for the extremity having changed its polarity, it will now repel the pole, N', and drive it onward, whilst at the same time i will be attracted by the next bar in succession. And in consequence of similar cnanges of polarity taking place in all the four bars, the pole, N', is kept continually revolving.

All that has been said about the pole, N', applies equally to the opposite pole, S', of the same magnet; so that by this means the magnet and its appendages are continually urged on by four forces—two attractions and two repulsions. And by considering that the lower magnet, N S, is by the contrivance also urged on at the same time, and in a similar manner, by four other like forces, it will easily be understood that the two magnets, with the shaft to which they are attached, are kept in motion by eight forces—four of which are attractive and four repulsive. Such is the contrivance for keeping the machine in motion.

To the upper end of the shaft is attached a vertical spindle, carrying an endless screw near its upper extremity, and revolving as the shaft revolves in a collar in the upper cross-piece. Near to the lower end of this spindle is a fly with three arms, equi-

distant from each other, and each terminating with a heavy brass crescent. It was originally the fly of a roasting jack. The endless screw works in the teeth of a brass wheel, also a part of the old jack. The arbour of this wheel runs in a frame attached at right angles to the upper cross-piece.

This engine was constructed in the autumn of 1832, and was exhibited for the first time in London, in the 21st March, 1833, in a lecture on Electro-Magnetism, which I delivered at the Western Literary and Scientific Institution; and, notwithstanding its then rude appearance, the Committee were so highly pleased with its structure and performance that they expressed a wish to have it brought forward again and hear it explained as soon as there was another opportunity. I was consequently honoured with an engagement to continue and extend my course of lectures in the following June; and in those lectures my engine again worked well, and excited a great deal of curiosity among the members of the Institution, and I believe was so fortunate as to give general satisfaction.

Since that time I have had attached to it contrivances for drawing water, waggons, and carriages on a railway, for sawing wood, pumping water, &c. upon about the same scale as we see pieces of machinery put into motion by the large models of steam engines. But as I saw several parts in which I thought it might be improved, it has long since been lain by, and another one is now building. The old one, however, is still in existence. W. S.

On Galvanic results, in letters addressed to Professor Silliman, October 4th, 1838, *and August 6th,* 1839, *from the vicinity of London.*

REMARKS BY THE EDITORS OF THE AMERICAN JOURNAL OF SCIENCE AND ARTS.

A very economical and efficient Voltaic arrangement was adopted by several members of the London Electrical Society, and the report of the construction and performance of the battery, in a series of experiments performed at Clapham Common, in the autumn of 1838, is contained in the report of Mr. Charles V. Walker, published in the Transactions of the London Electrical Society, in two papers, dated October 16th, and November 6th, 1838:—

"A Voltaic battery has been got up (at the expense of two of our leading men, whose names I am not at liberty to mention), for the sole purpose of investigation. The battery consists of one hundred and sixty porcelain pint jars, each containing a copper and zinc cylinder: the latter, being covered with stout brown paper, is introduced into the interior of the copper. The exciting fluids are solutions of sulphate of copper and muriate of soda: the former applied to the copper cylinders and the

latter to the zinc ones. When the jars were in series, the flame, which was upwards of an inch long from a charcoal point, rotated on the poles of a magnet, according to principles of Electro-Magnetism. Davy *deflected* the electrical flame by magnetic influence, but I am not aware that he *rotated* it.

"Sulphuret of lead (galena) was decomposed, and metallic lead obtained. Sulphuret of antimony was decomposed, and the liberated metal kept in fusion for several minutes. The boiling antimony was *three inches long* and half an inch wide between the polar wires, and exhibited a beautiful spectacle, in a channel of those dimensions, which the action had formed in the native sulphuret. When the electric flame was directed through the air between stout copper polar wires, the positive wire became red-hot, but the negative wire could not be made red. The wires were made to change poles, still the same thing occurred; nay, even two inches of the positive wire, which was completely out of the circuit, was rendered hot, but no redness appeared on the negative wire. How exceedingly curious and interesting is this last result!

"When the whole battery was formed into eight groups of twenty jars each, and properly connected with an electro-gasometer, the mixed gases were liberated from water at the rate of one cubic inch per seven seconds; and this for many successive minutes, although the battery had been in action for seven previous hours without interruption."

In his letter of August 6th, 1739, Mr. Sturgeon proceeds to observe, that a good description of the apparatus and experiments will be found in the Memoir above named, and of which he kindly transmitted a copy. But he remarks—"There are some particulars connected with the discovery of the *difference of temperature*, produced in the positive and negative wires, which want a clearer description than any given by Mr. Walker, or perhaps any which that gentleman had then a means of giving; and as I find, from the defective information which has been given of this particular discovery on the continent of Europe, that M. de la Rive and others have failed in reproducing the curious phenomenon, it is possible that the American philosophers may also fail from a like cause, were the particulars of manipulation not made known to them. I will, therefore, for the information of all the readers of your excellent journal, give a brief historical sketch of the whole business."

"The battery consisted of a hundred and sixty white porcelain jars, each of the capacity of about two thirds of a pint, and furnished with a hollow cylinder of sheet copper, and an interior hollow cylinder of sheet zinc, in metallic connection with the copper of the next pot, &c. The copper and zinc of each pot were separated from each other by a diaphragm of brown paper (a disc, on the centre of which is placed the centre of the base of the zinc cylinder, and the periphery brought up to the upper end of the latter, so as to form a bag round the zinc), which separates the solution of sulphate of copper, which is placed *outside*, from the solution of common salt, which is

placed *inside* of it. Hence the copper is washed with its sulphate solution, and the zinc with the muriate of soda solution.

"One hundred of these metals and pots were furnished by Mr. Gassiot, and the other sixty by Mr. Mason. The preparation of a battery of this kind and extent is a great labour, as you will undestand from the following particulars:—Mr. Walker commenced working at it between eight and nine in the morning; Mr. Mason arrived about eleven in the forenoon, and immediately set to work at it; Mr. Gassiot commenced shortly afterwards, and it was not ready for experiment till three in the afternoon, about an hour and a half after I arrived at Mr. Gassiot's house. The plan of dividing the battery into groups for the experiments on decompositions was formed by Mr. Mason, who is a skilful and very neat Experimenter.

"At a previous meeting I was requested to provide a catalogue of experiments, which I did, but in consequence of the great length of time occupied in the decomposition of water by the various forms of the battery, only a few of them were attempted. As the decompositions are very well described by Mr. Walker, it would be unnecessary to say anything more about them in this place. They were carried on with great exactness in the following manner. The graduated glass tube of the electro-gasometer being filled with acidulated water, and inverted over the platinum terminals of the instrument, one of the polar wires of the battery was connected with it, and the other kept in the hand of the Experimenter, ready to plunge into the other mercurial cup of the instrument the moment the word 'time' was given, and taken out again when a cubic inch of the gases was collected.

"With regard to the experiment in which I discovered the great difference of temperature produced in the two polar wires, it was undertaken from the views which I had long entertained concerning the non-identity of the *electric* and *calorific* matter, as you will see I have hinted at, at the close of Part I. of my first Memoir to the London Electrical Society.* It was late in the evening before I had any opportunity of making the experiment. The rest of the party were engaged in something else at the time, and the battery was in series of one hundred and sixty pairs. I brought the tip ends of the polar wires (copper wire one-tenth of an inch diameter) into contact, end to end, then withdrew them gently and very gradually from each other, keeping the flame in full play between them, till they were separated about one-fourth of an inch. In a few minutes the positive wire got red-hot for half an inch, but the negative wire never became red. I repeated this several times in order to be convinced of the fact. I next laid the wires across one another, and brought them into contact about an inch from the extremities, and separated them as before. In a short time the whole of that part of the positive wire from the point of crossing to the extremity became very red-hot, but the negative end never got even to a dull redness: it was certainly very

* See twenty-second Memoir, page, 386.

hot, but never higher than a black heat. I next increased the length of the ends of the wires exterior to the circuit, and eventually heated two inches of the positive wire to bright redness; but no such heat took place on the other wire. Thus satisfying myself that I was not mistaken, I called Mr. Mason to come and look at it; and after satisfying that gentleman by an experiment or two, we called Mr. Gassiot and Mr. Walker to come and witness the novel phenomenon. We now changed the places of the polar wires, making that positive which before had been negative, &c.: still the positive wire showed the same fact. You will easily understand that I experienced a great degree of pleasure at the appearance of this beautiful fact, which seemed to demonstrate the justness of the hypothesis I had so long formed. *No two bodies can be in the same place at the same time* is an old axiom in philosophy; hence the blacksmith is enabled to heat his iron rod or nail by compressing the calorific matter, by the blows of his hammer forcing it from the *cavities* into the *particles* of the metal. Thus also the electric fluid forces the calorific matter from its natural lodgings in the conductor, and drives it on even beyond the electric stream, to take refuge, in a compressed form, in the extremity of the positive wire. Nothing can be more simple to explain; nor do I know of an experiment that tends more to support the doctrine of *one species* of electric matter only, and that it runs through the Voltaic conducting wires *from* the positive *to* the negative pole.

"To produce the phenomenon I have been describing requires an extensive series of pairs; certainly not less than one hundred and twenty, but two hundred would answer much better, as much depends upon the play of the fluid between the wires; and I think that the battery is quite as well when not highly charged. I have mentioned one hundred and twenty as the shortest to insure success, although it is possible that one hundred might show the fact."

The following remarks, in answer to inquiries made of Mr. Sturgeon, as to his views regarding the best forms of Galvanic batteries, are worth preserving, as the conclusions of so experienced an Experimenter, and more so as they coincide generally with the views of Dr. Hare and of other distinguished men in this country.—*Eds.*

Form and Size of Galvanic Batteries.

"With respect to Galvanic batteries, we can never expect to find *one* which will exhibit every class of phenomena to the best advantage. The pile, with moistened card-board in pure water, or a well-constructed Cruickshank, charged with water, answers best for charging Leyden jars, deflections of pith balls, &c.; and the more extensive the series the better. The size of the plates has also much to do in this

business. A single pair of plates, charged with dilute nitrous acid, answers best for most electro-magnetic experiments. For a display of *brilliant* calorific phenomena, the burning of charcoal, deflagration of laminated metals, &c., a series of not less than a hundred pairs answers better than any smaller series. Here again the size of the plates should never be less than four inches square. Six inch plates answer much better, and two hundred better than one hundred, &c.; and these may be either of the Cruickshank form, or of any other, observing that the action with the former is of much shorter duration than with the Wollaston form, and shorter with the Wollaston than with the battery before described.

"Then again for heating of thick wires, a series of ten, or less, of *large plates*, are better than more extensive series.

"For chemical decompositions, there is, perhaps, no battery known so well adapted for them as the jars which I have described. There sustaining power is a great recommendation. The extent of series will necessarily vary with the nature of the compound operated on. We have found that a series of twelve jars give a sufficient intensity for the decomposition of acidulated water (water, 10, sulphuric acid, 1, or even much less). Twenty-four jars, in a double series of twelve, give about twice as much gas as a single series of twelve; but twenty-four jars in a single series do not give so much gas as when they form a double series of twelve. Again, thirty-six jars, in one series, do not give so much gas as when they are formed into a treble series of twelve. Hence a series of twelve of *these* jars seems to be about the best *unit of intensity* for acidulated water. Other compounds will require other *units of intensity* to produce maximum effects, and other batteries will require different extent of series to produce the same *unit of intensity* as that produced by the jars."—*Letter to Professor Silliman (from Silliman's American Journal of Science and the Arts.)*

THE END.

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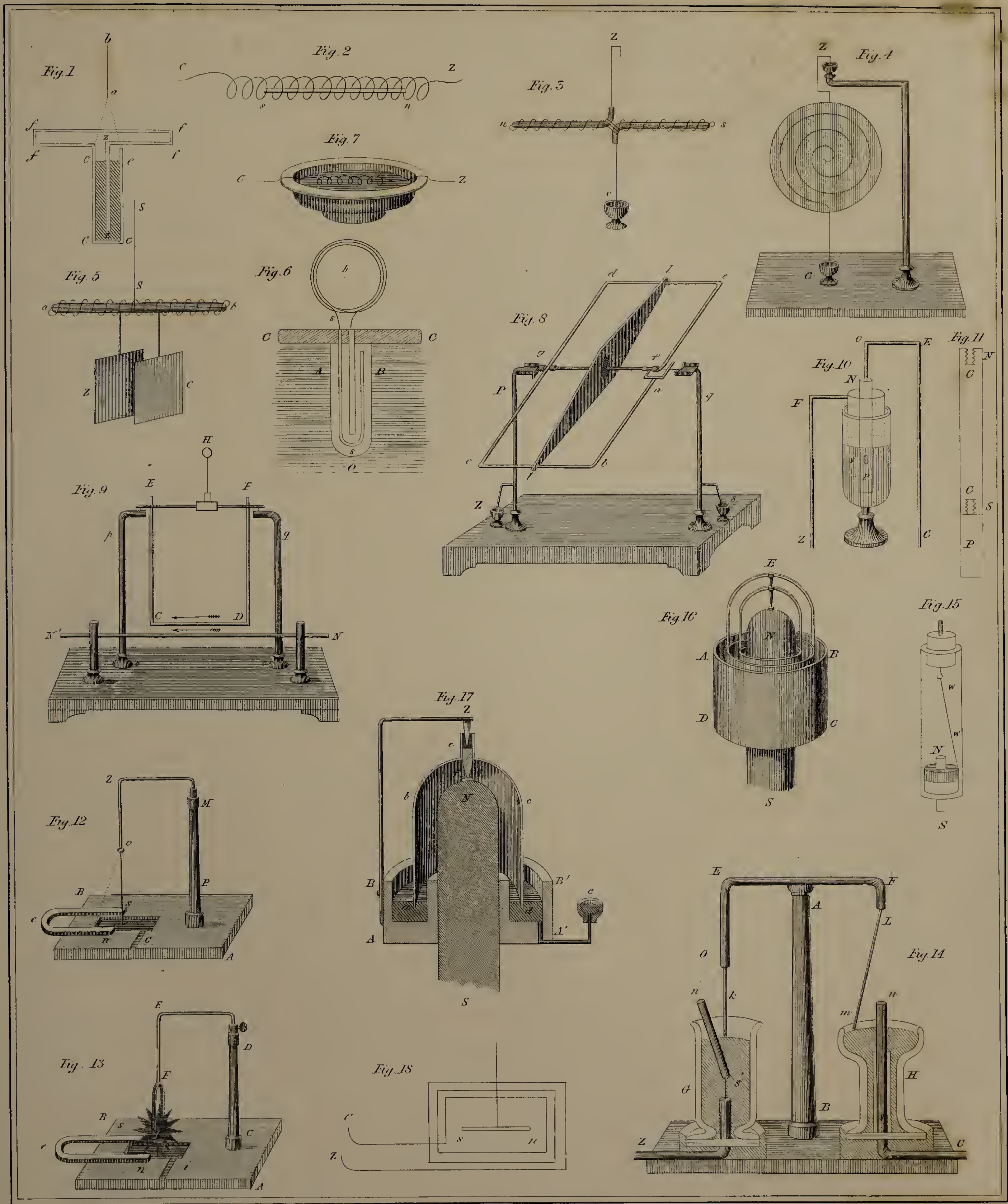
Note to page 331 *and Fig.* 15, *Plate XIII.*—There are only two of the eight magnets shown in the figure. The system is enclosed in an equatorial band of brass, against which the spring, P, presses, to keep up the contact between the cup c, and the axial wire of rotation. The band is slightly grooved for the reception of the spring, P, and lubricated with sweet oil.

Crompton, Printer, Bury, Lancashire.



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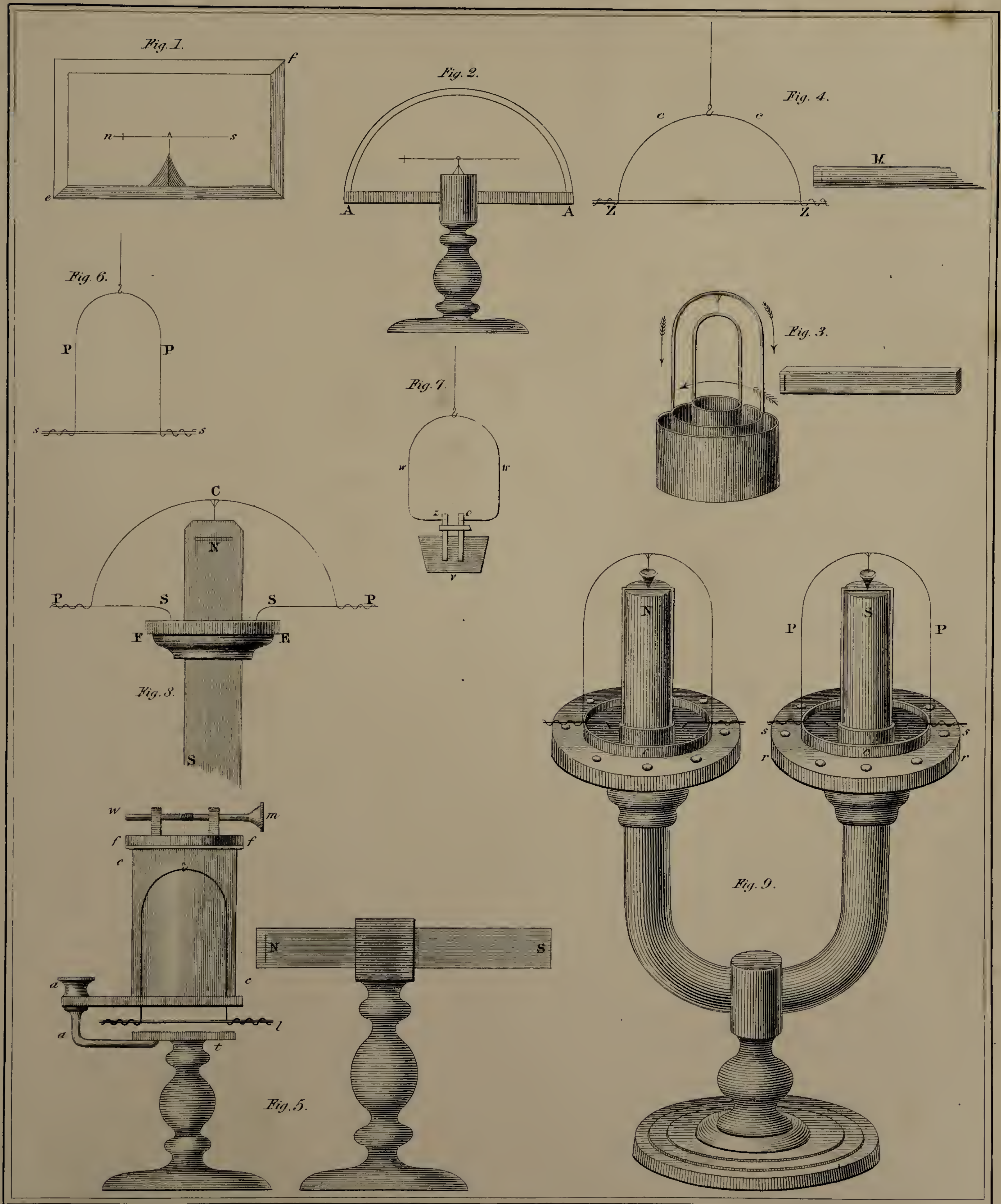
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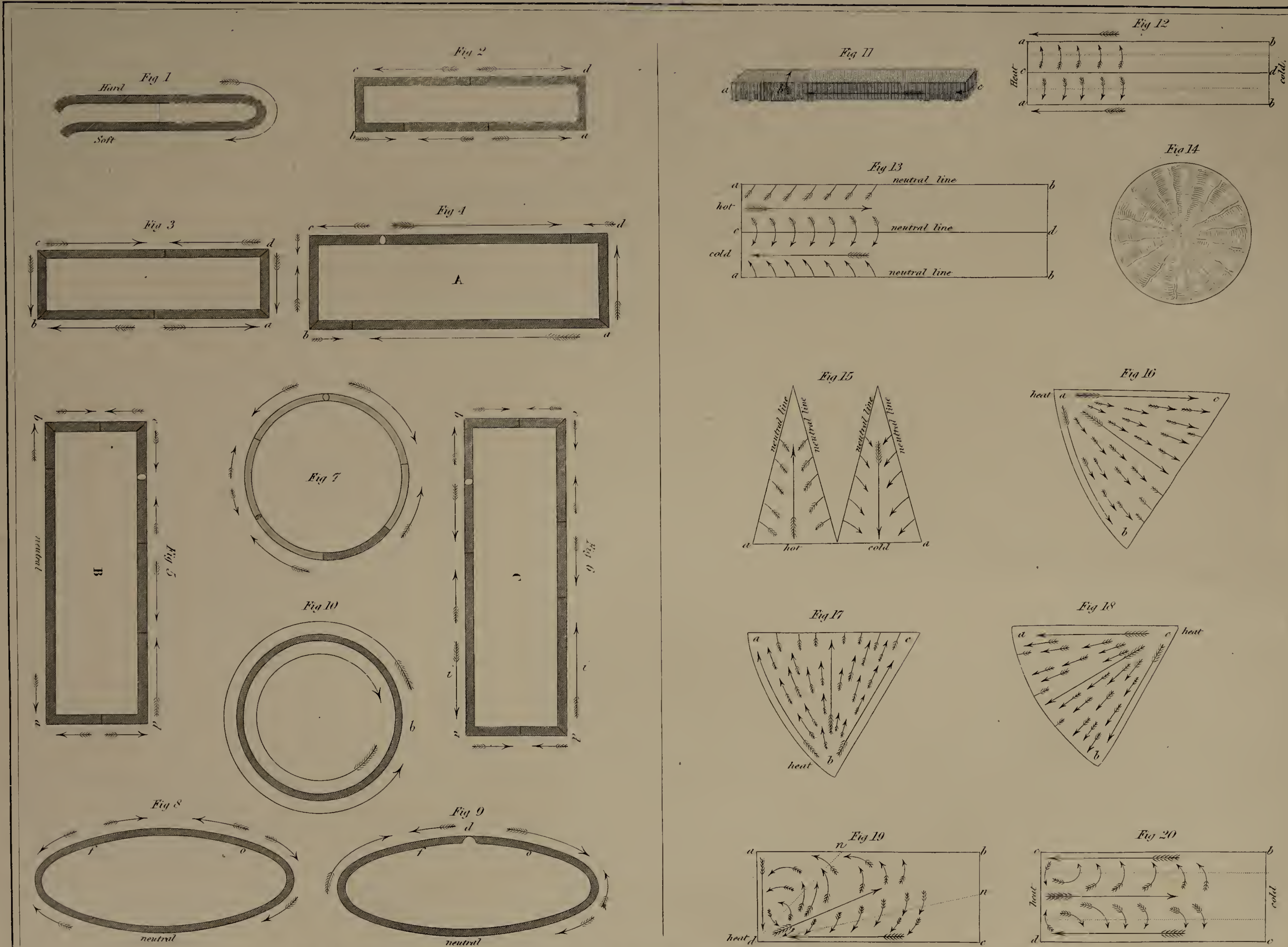

Mr. Sturgeon's Experiments on the Thermo-Magnetism of Simple Metals.

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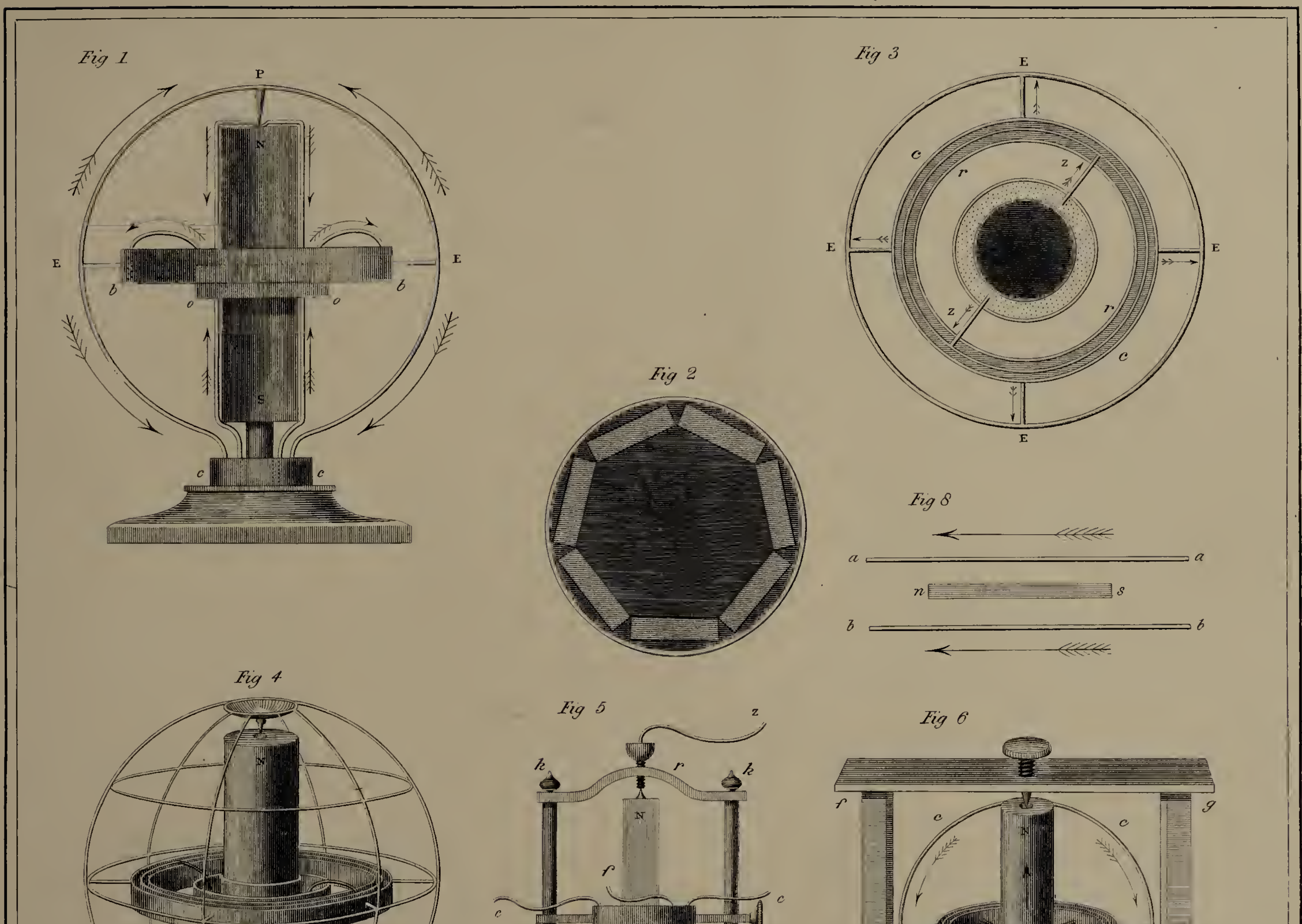

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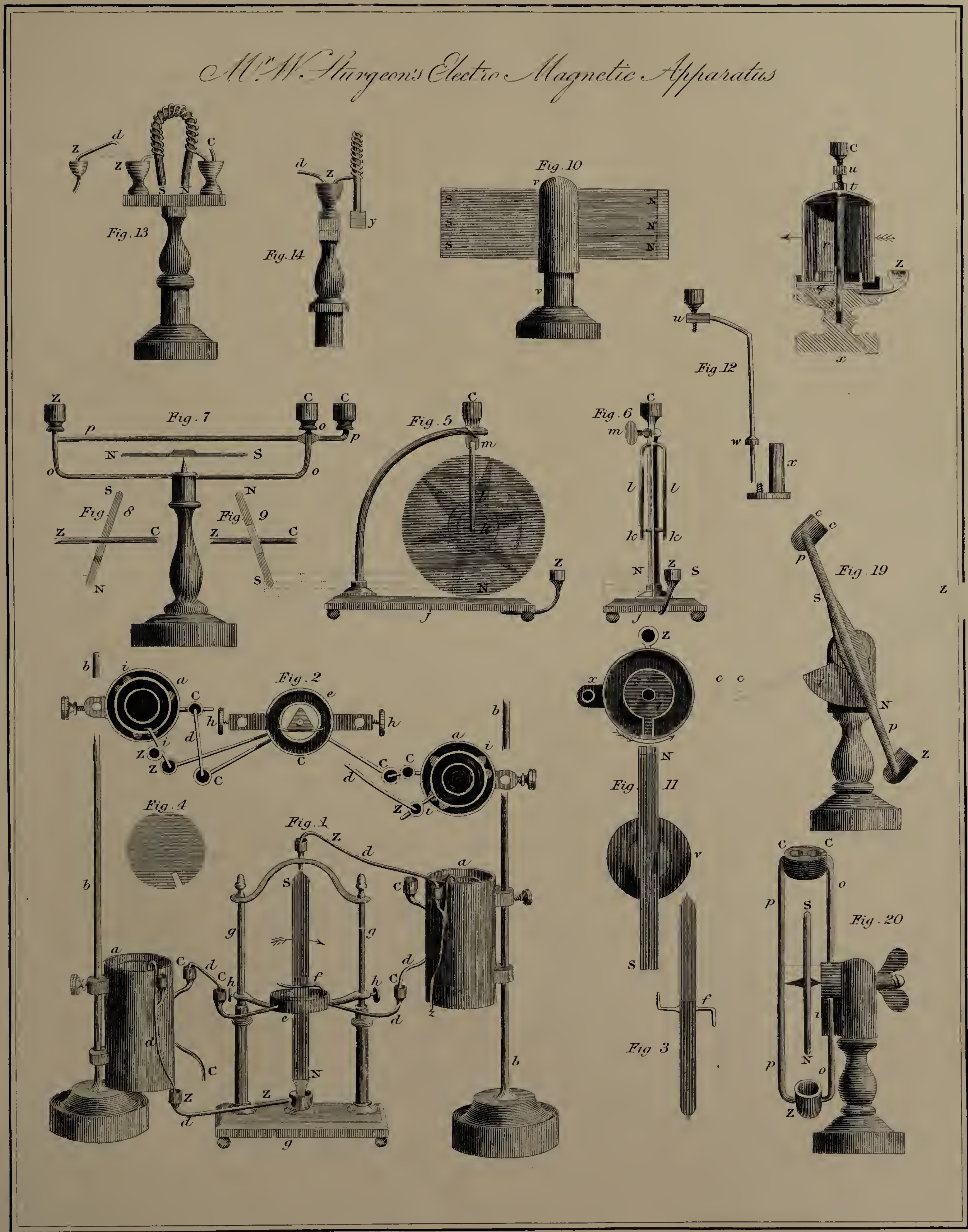
Mr. W. Sturgeon's Electro Magnetic Apparatus
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Fig. 2
Fig. 4
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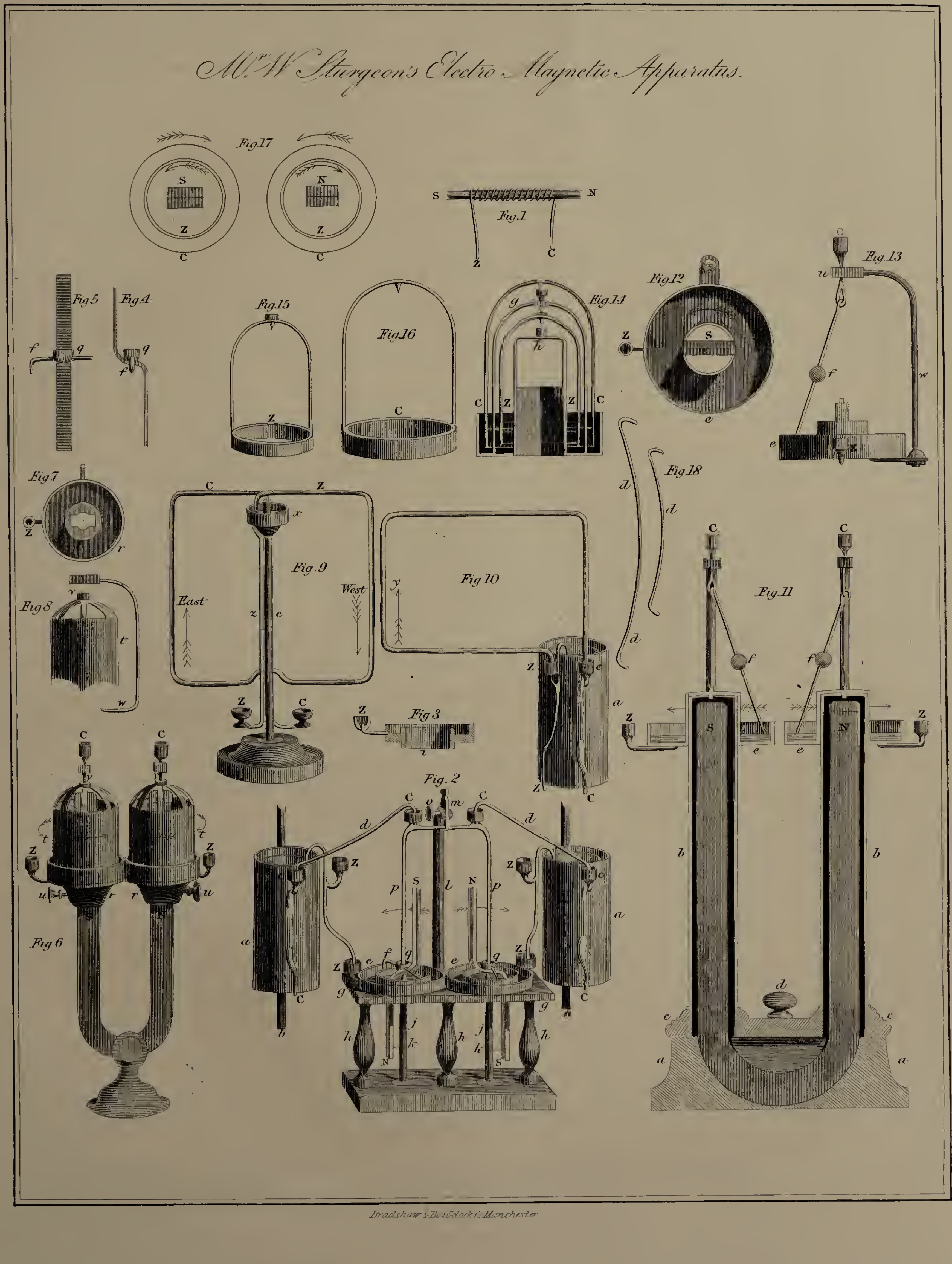

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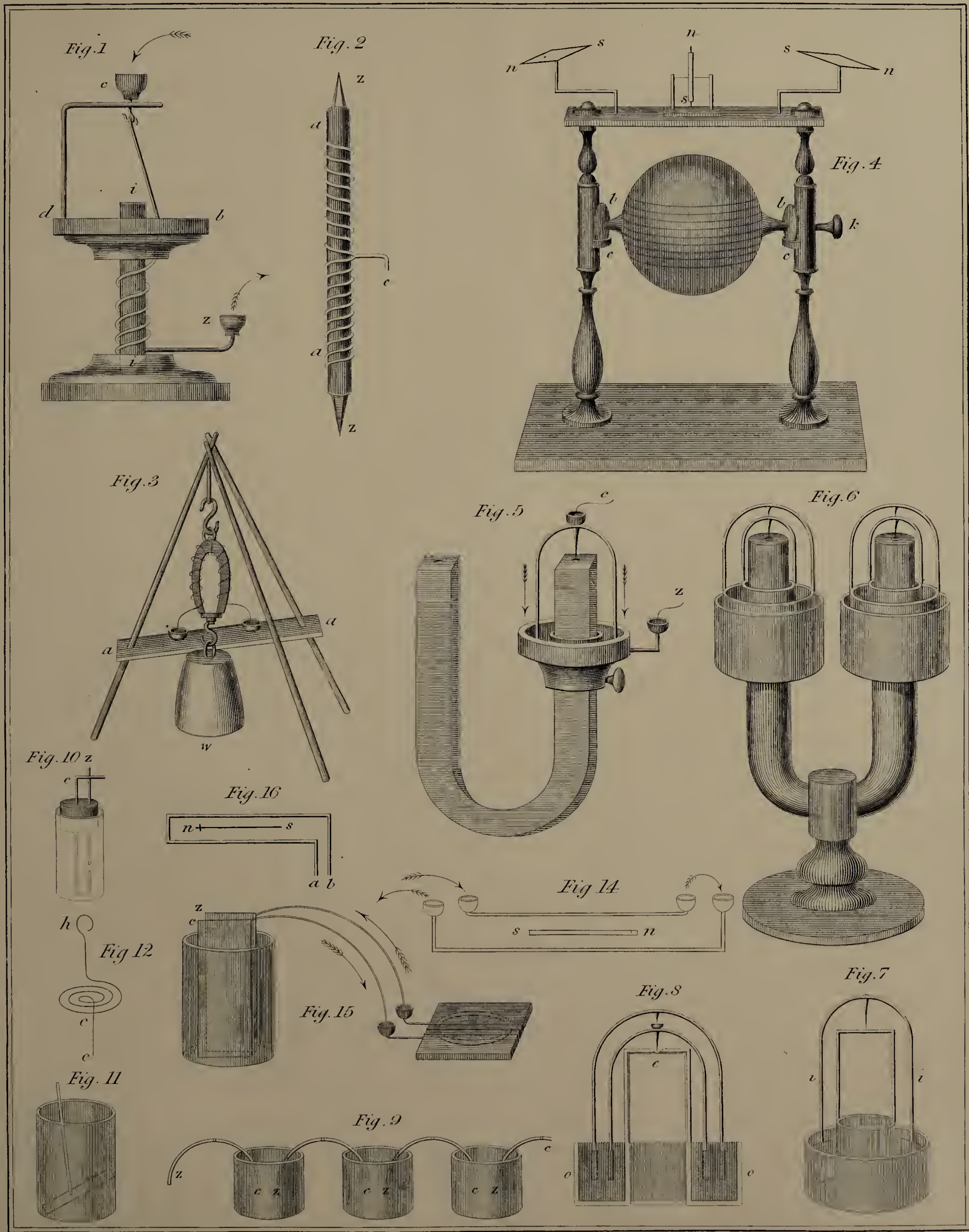
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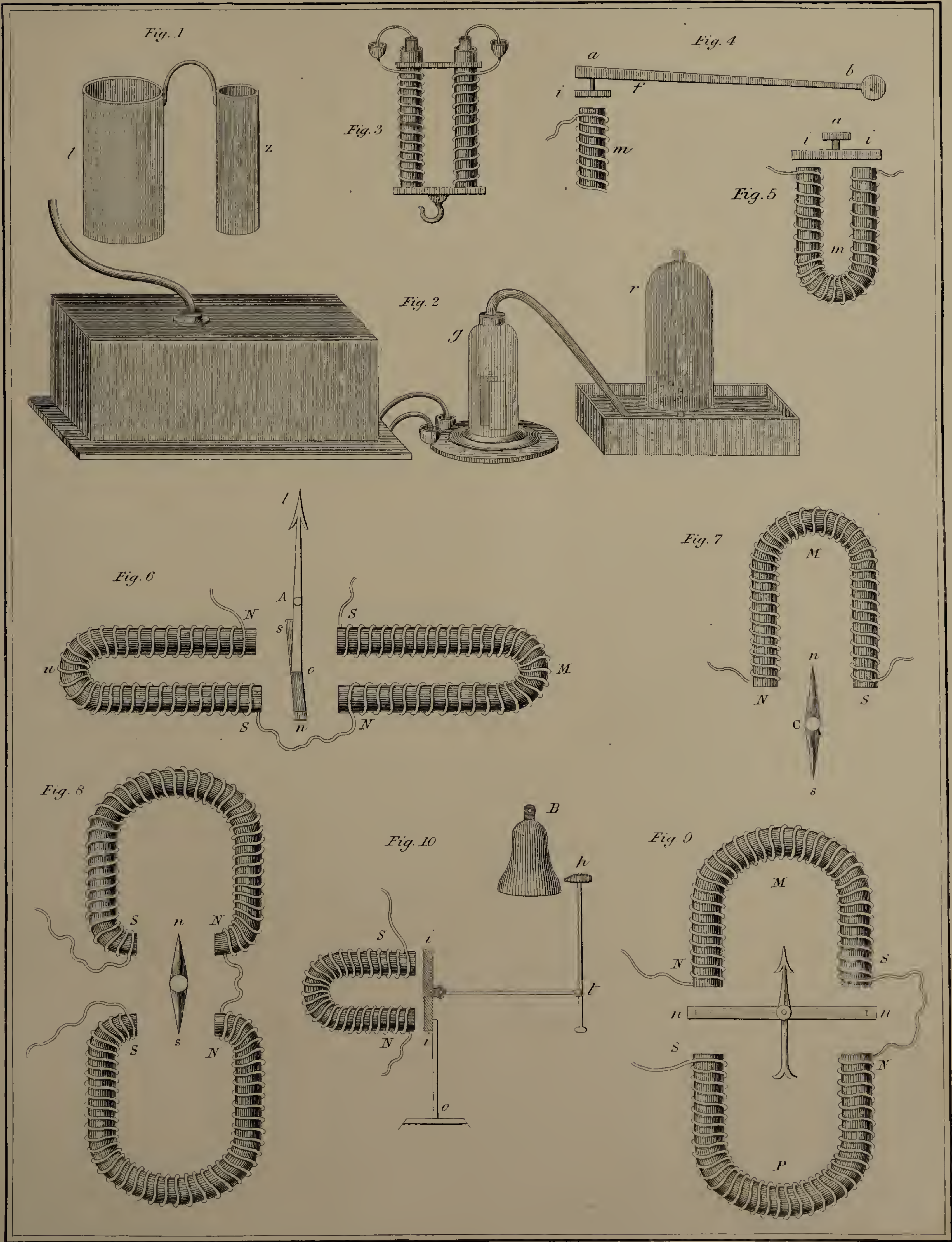
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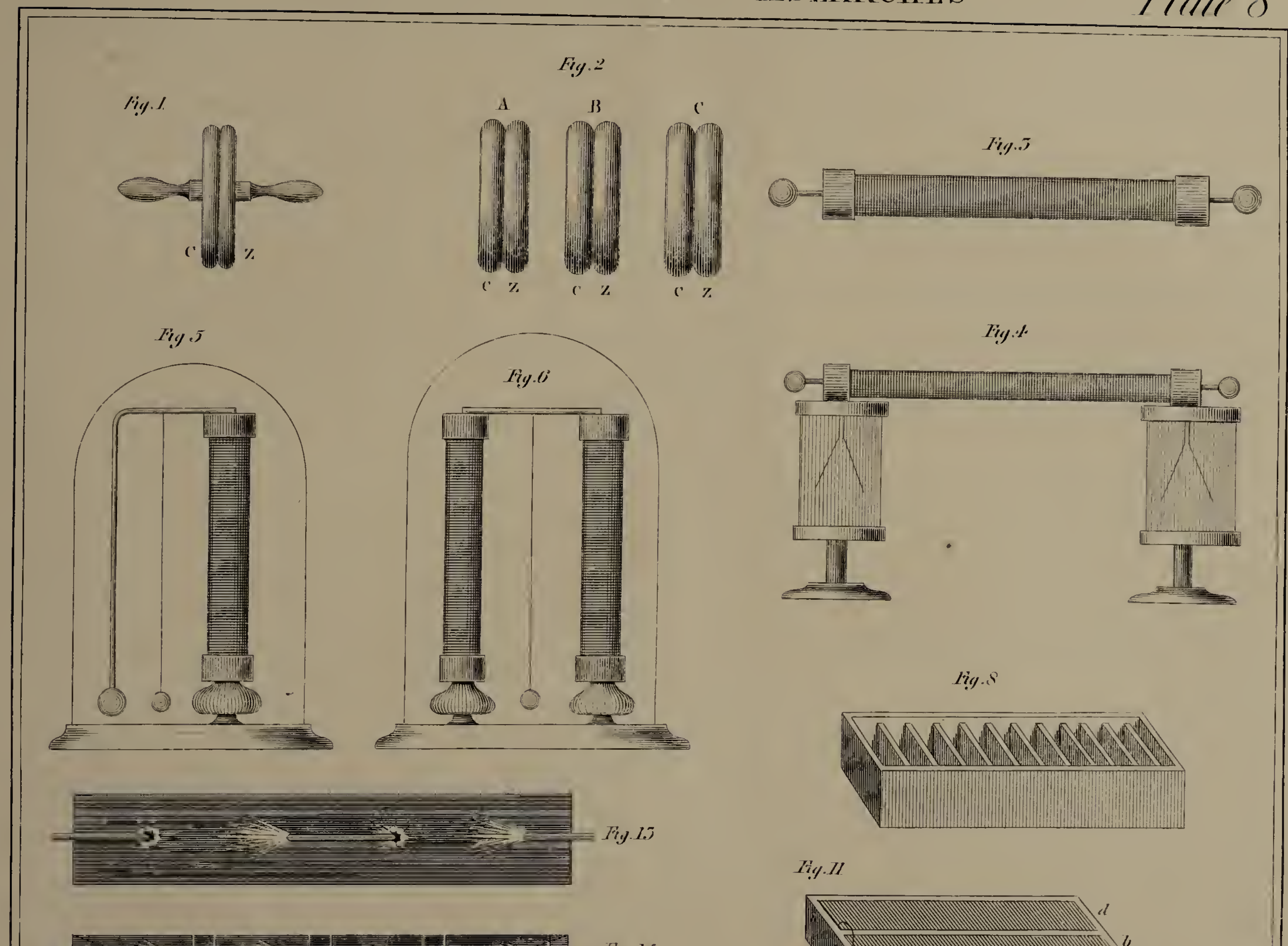

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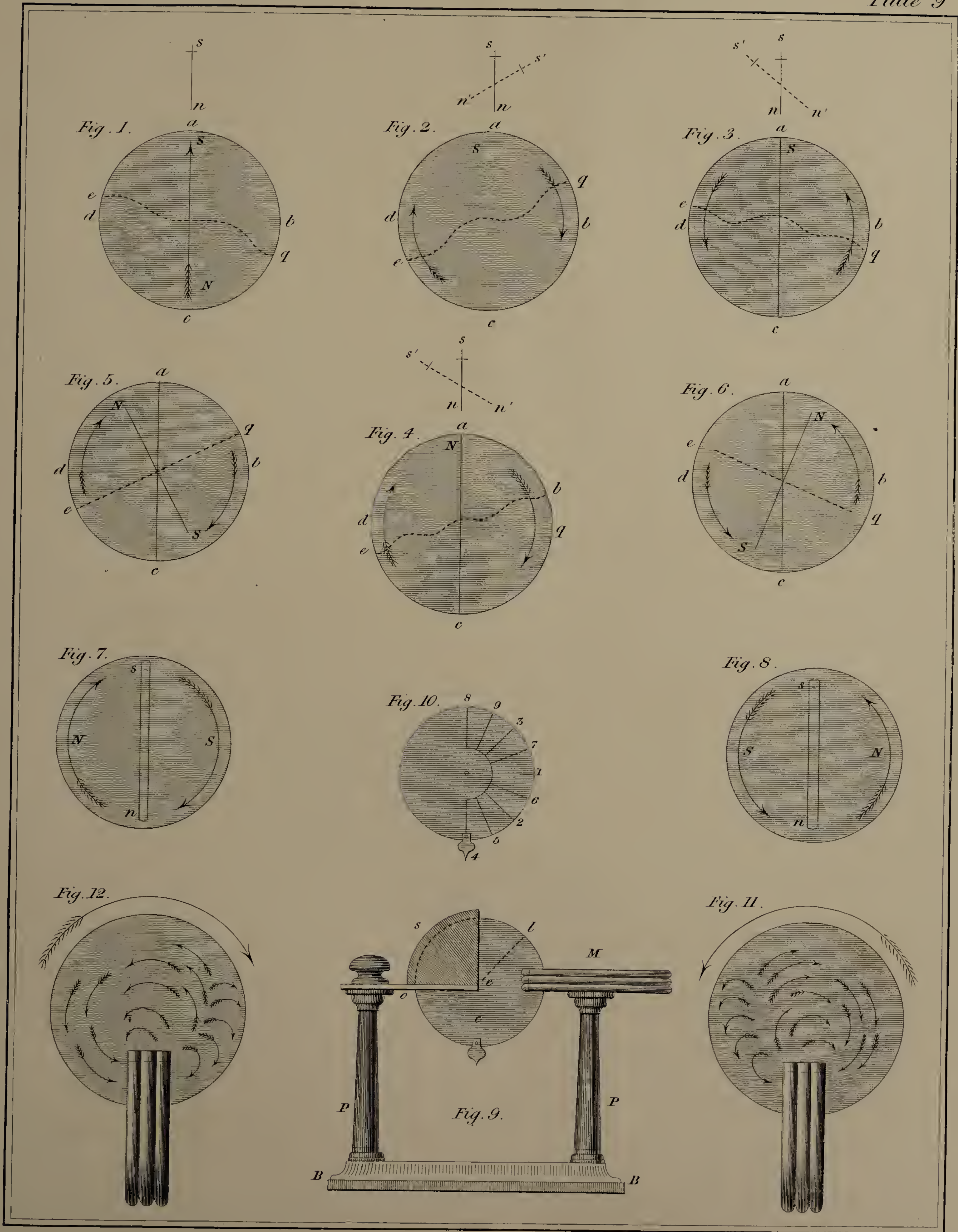

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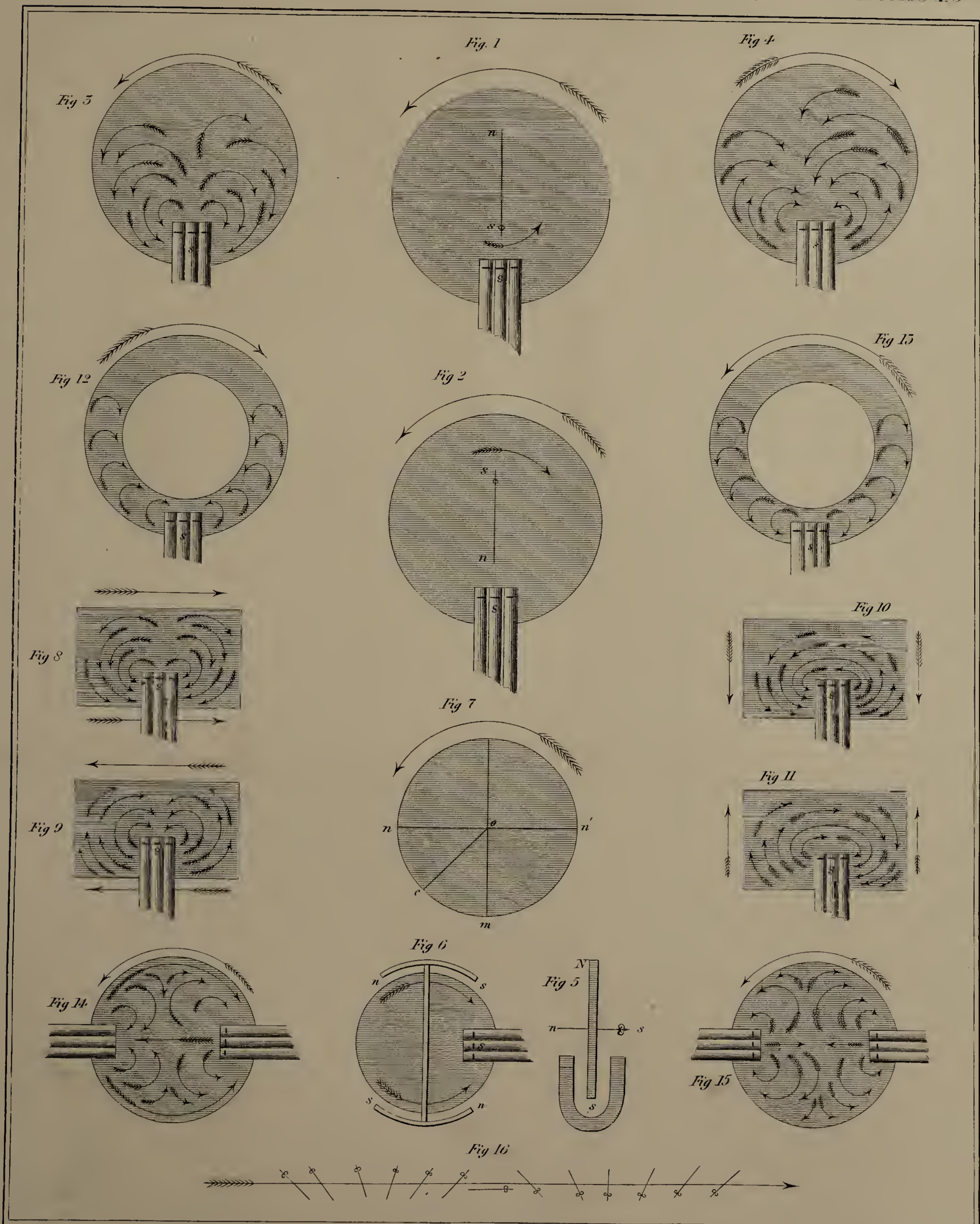

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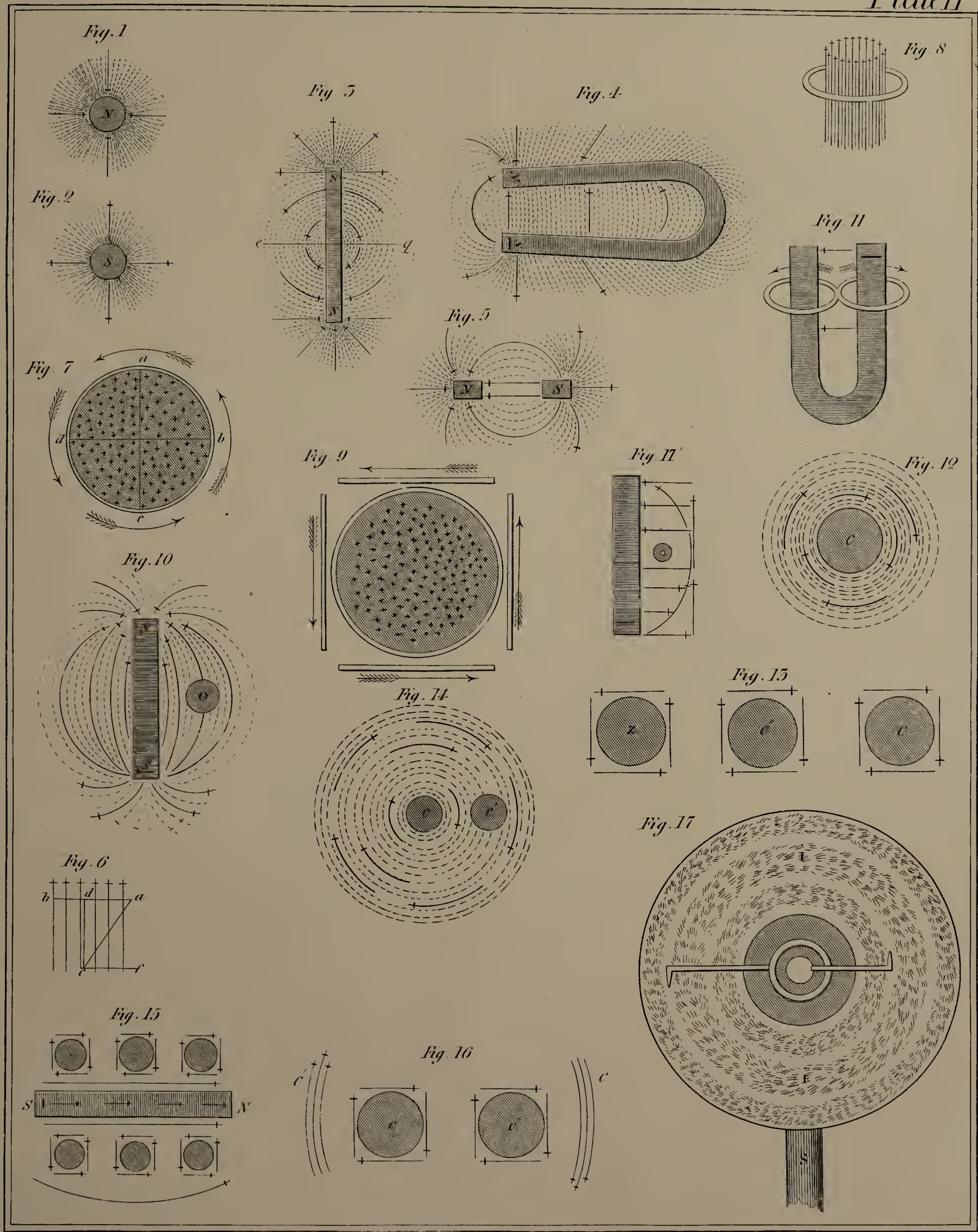
STURGEONS SCIENTIFIC RESEARCHES

Plate II



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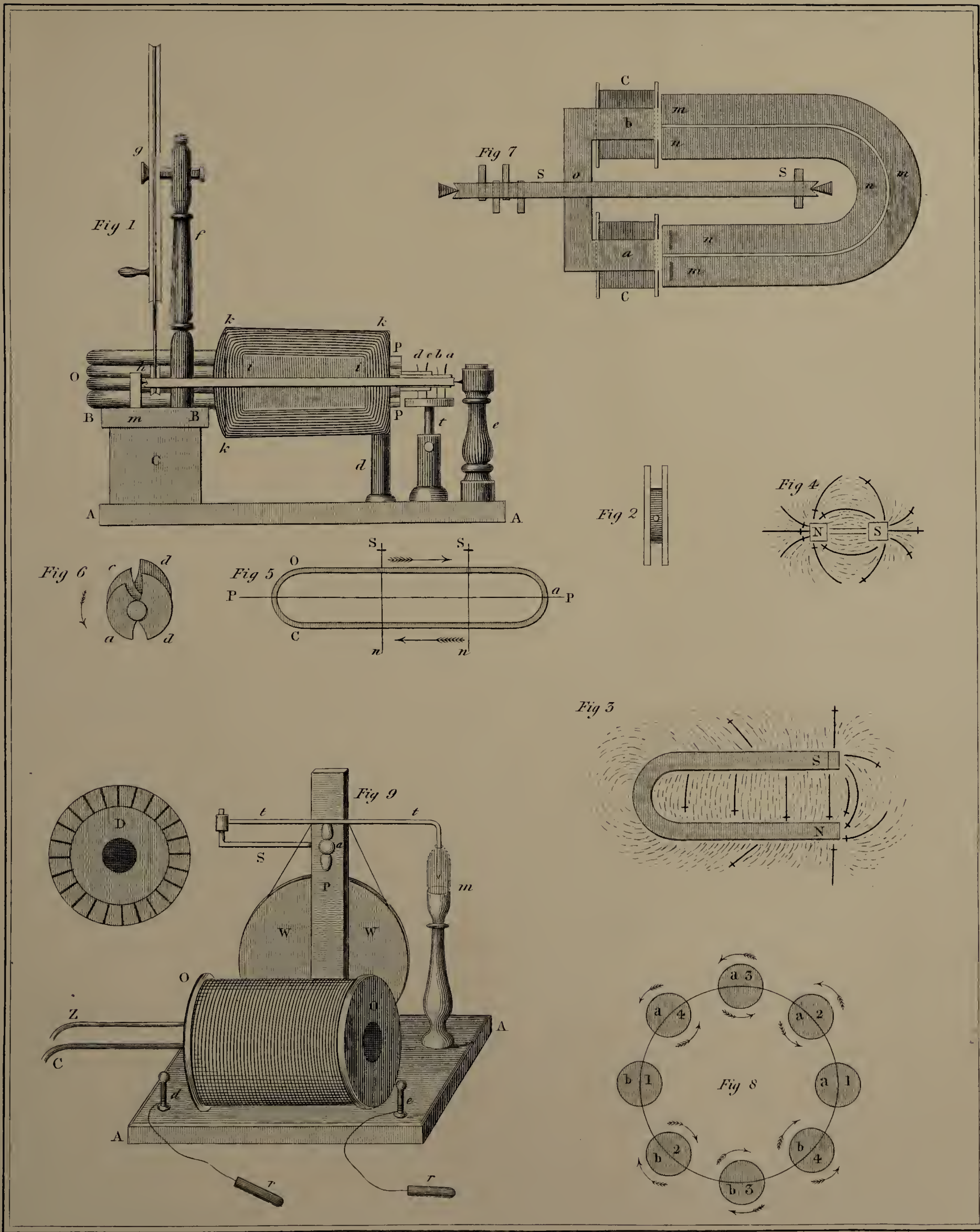

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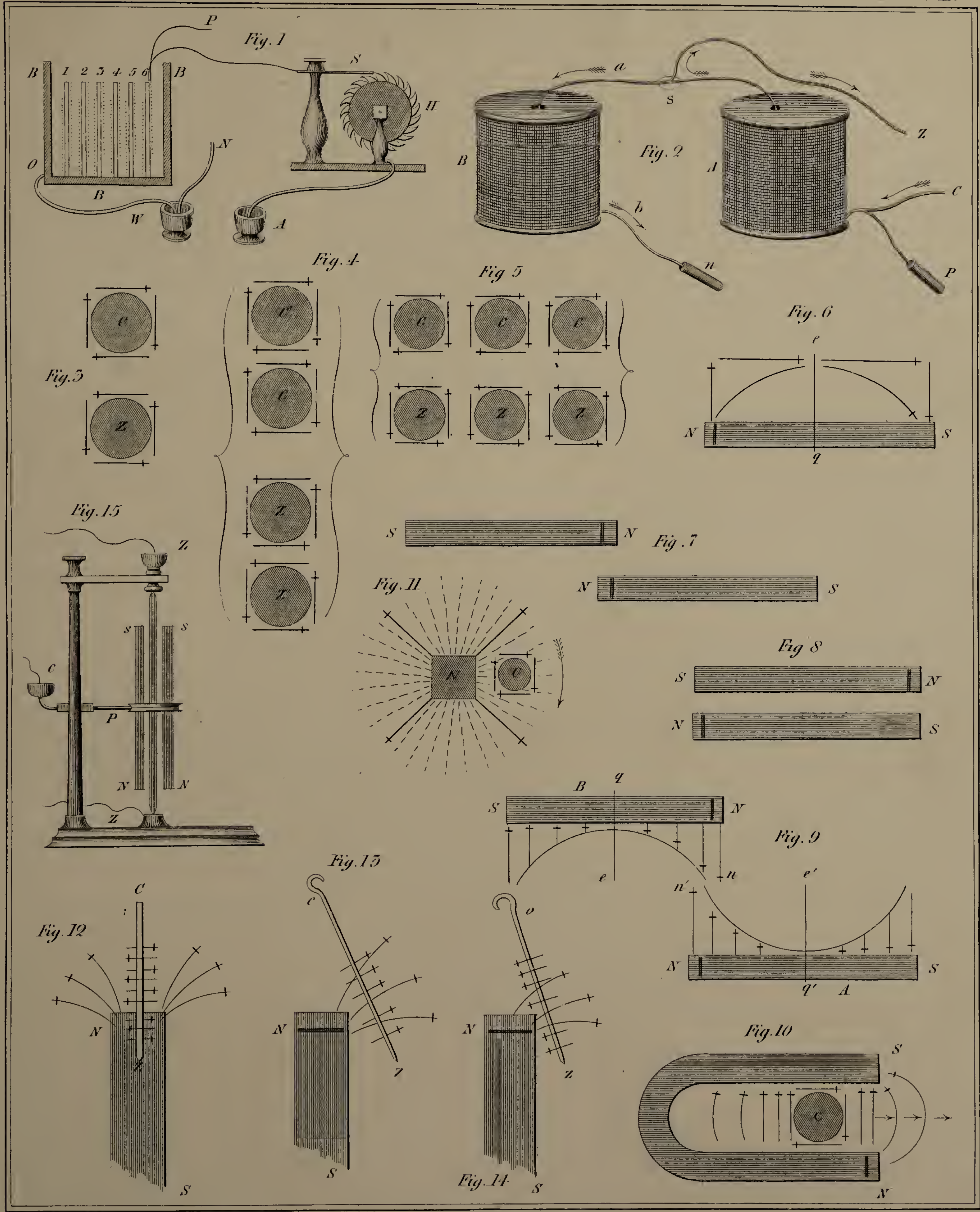

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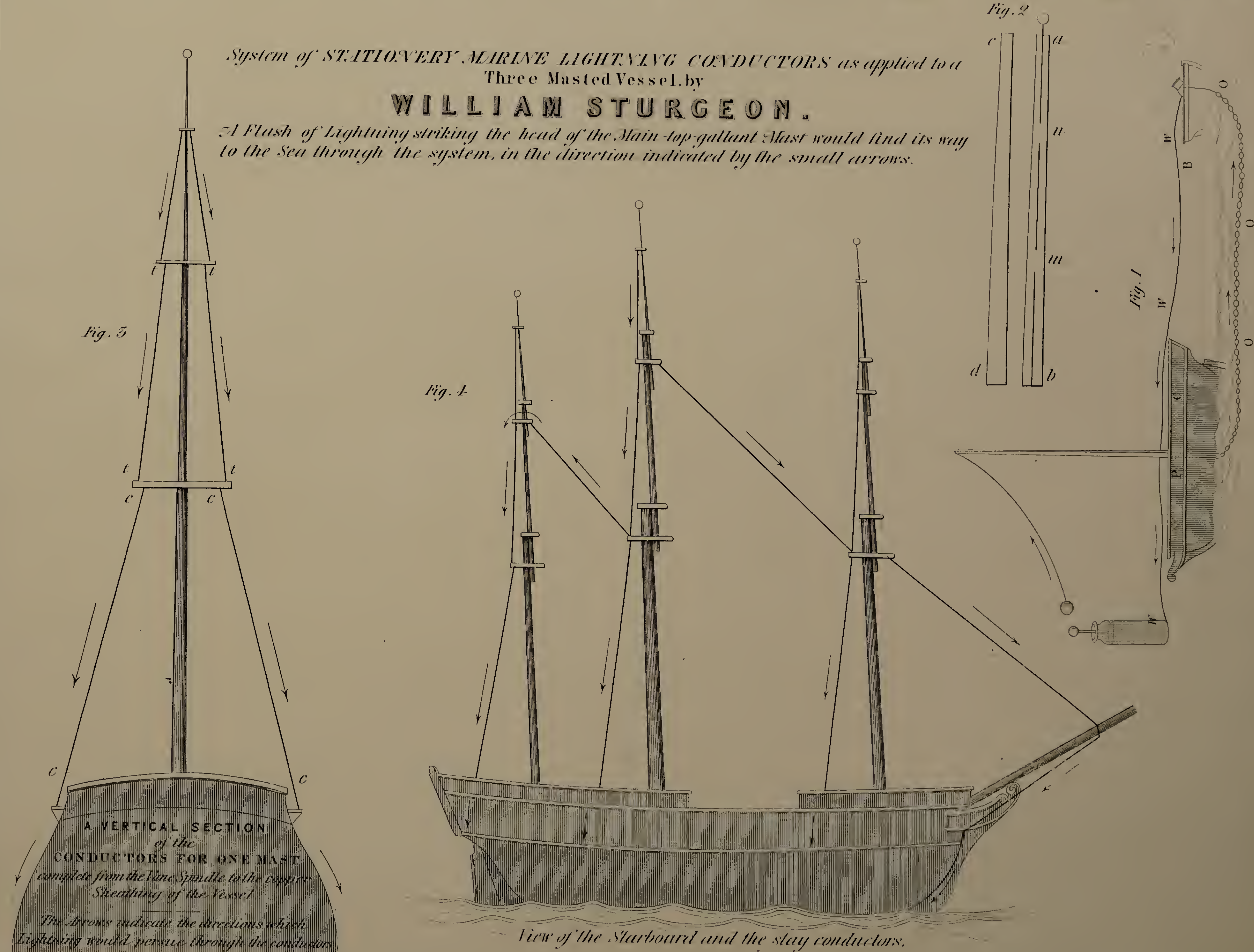
System of STATIONERY MARINE LIGHTNING CONDUCTORS as applied to a Three Masted Vessel, by

WILLIAM STURGEON.

A Flash of Lightning striking the head of the Main-top-gallant Mast would find its way to the Sea through the system, in the direction indicated by the small arrows.



View of the Starbourd and the stay conductors.

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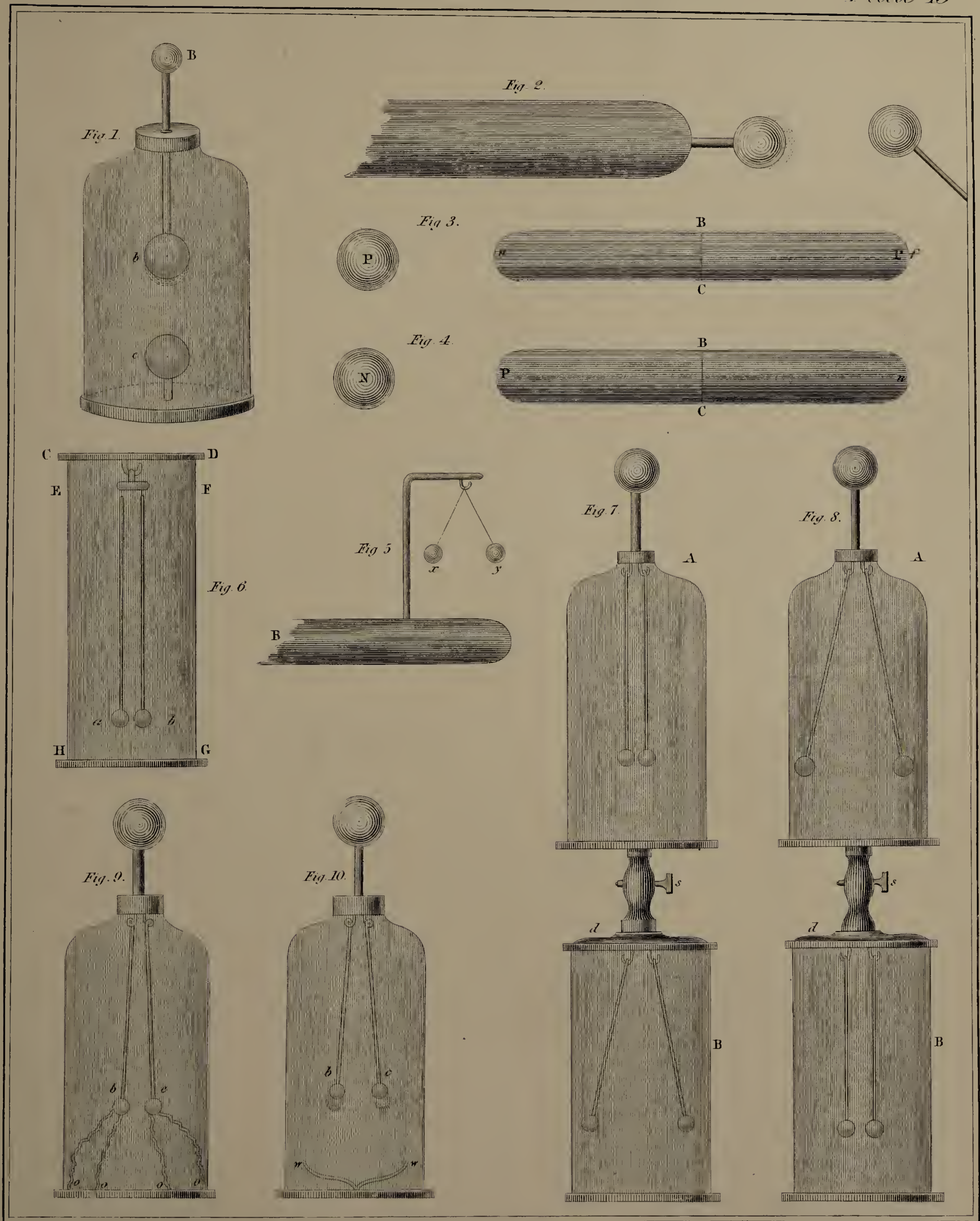

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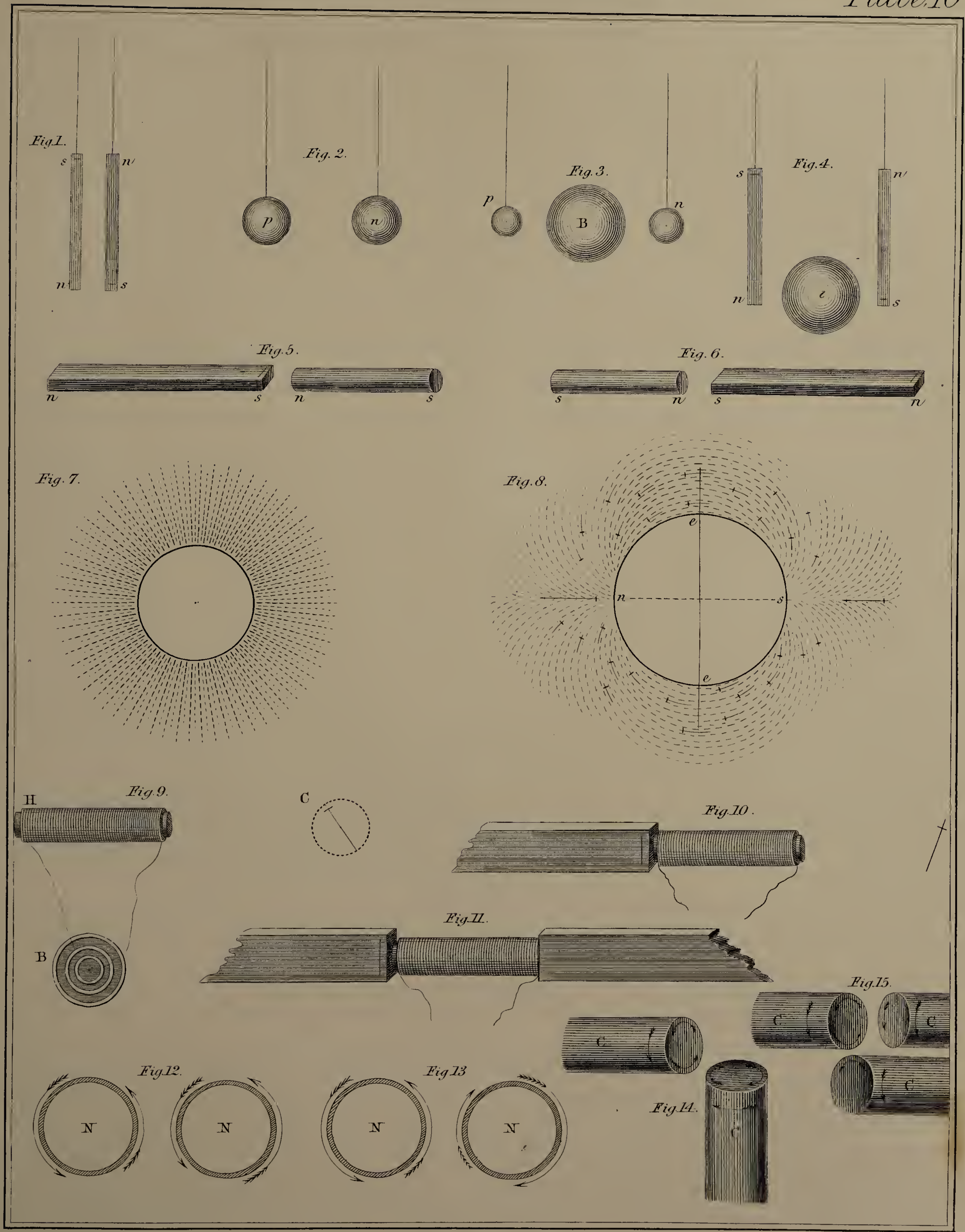

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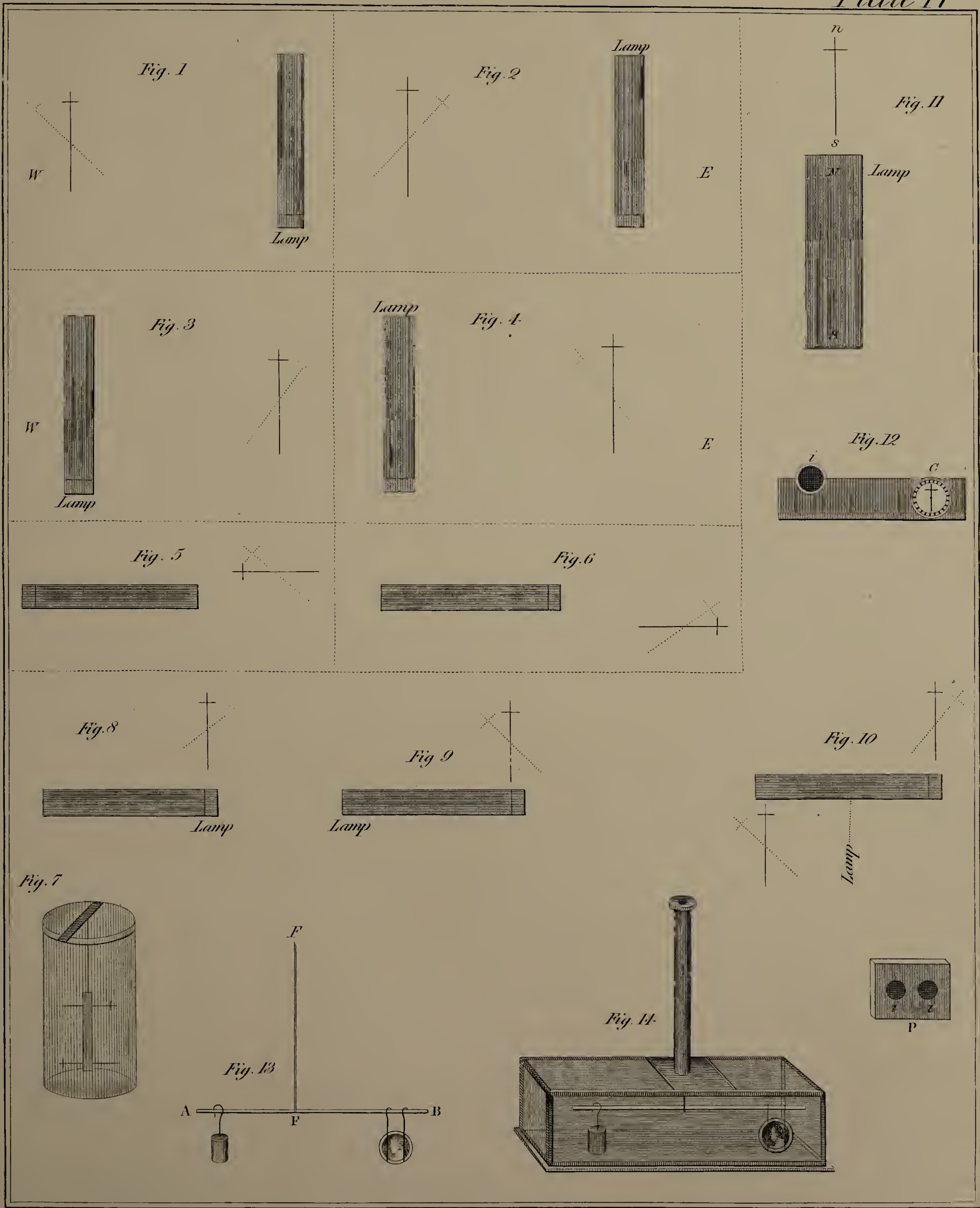
Plate 17



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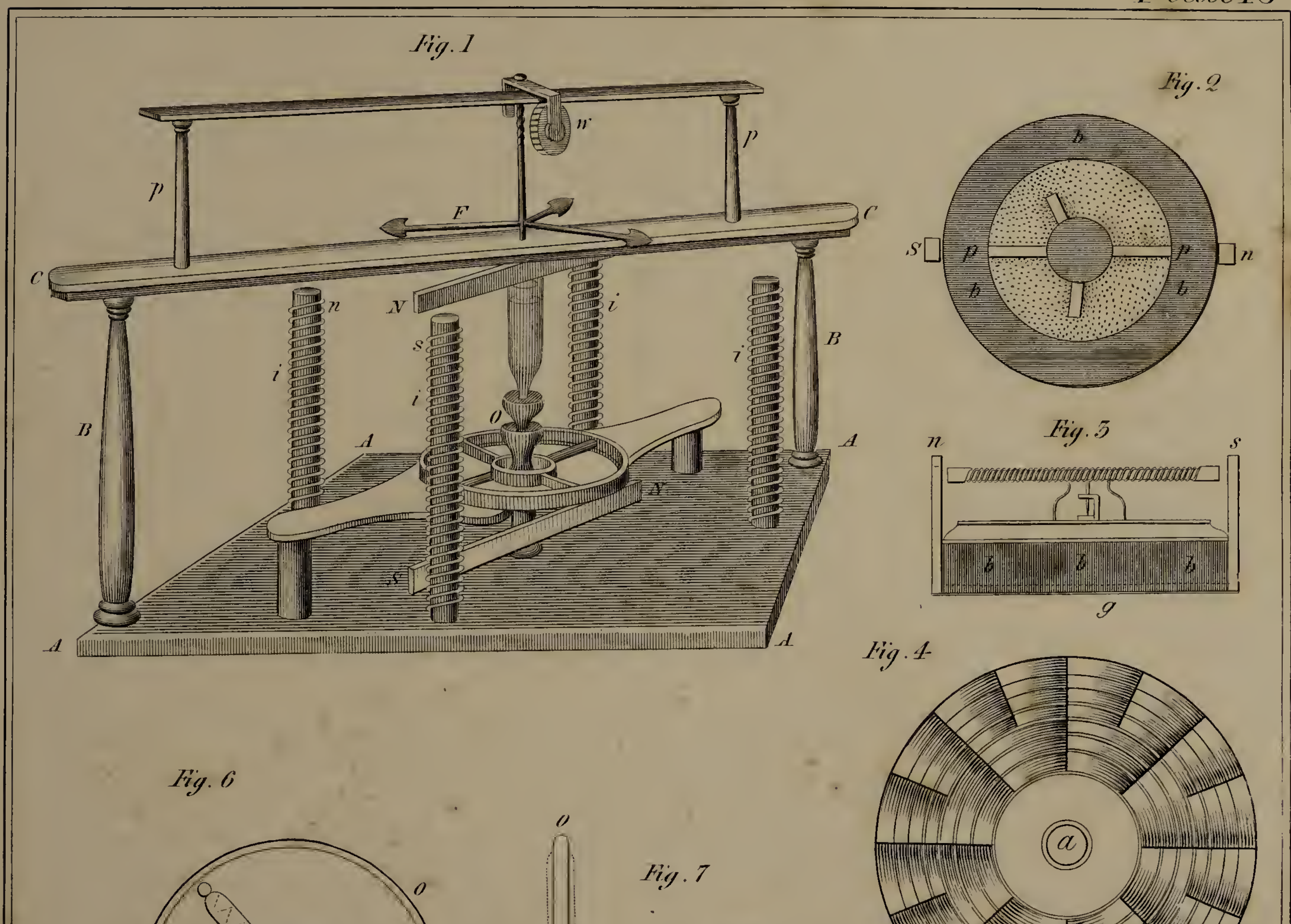

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